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ORIGINAL COMMUNICATIONS.

[The following communication relates to a subject of great im portance, especially at the present time. The author is one who loves his profession, and feels a deep interest in all that concerns its honor and usefulness; and whatever may be thought of the practicability and expediency of his scheme, all must admit that some improvement in the present state of things is desirable. Whether it is possible to carry reform to the extent proposed by our correspondent, in our present political and social condition, may well be doubted; the subject, however, is one eminently deserving of consideration.]

MEDICAL REFORM.

At this moment when there are in Philadelphia four chartered institutions, and two voluntary associations, in all of which complete courses of medical Lectures are, or will be, delivered; when we have reason to believe that arrangements are in progress for enlarging still further the corps of public medical instructors; and when, moreover, the number of students resorting to this city appears to be rapidly increasing;—ought not those who stand at the head of the profession to consider the time peculiarly opportune for making the improvements in medical education which all have long admitted to be useful, but none have yet had the resolution to adopt? The present state of things cannot be followed by the ancient *statu quo;* there must be a movement in

in one direction or the other. The existing competition in teaching must result either in the subsidence of all the schools to a level with the lowest, or in the extension and elevation of some one among them to such a degree as to defy all rivalry, and such as will furnish the student with an education fitted to keep him up with the march of medical science. As sure as fate a change must come. Let us hope that it will be commenced voluntarily, and with a good grace, by the institutions that have hitherto held an admitted and deserved pre-eminence. Let them not wait till it is thrust upon them, until their foundations are upheaved by the pressure from below, until the glory of reform is obscured by the necessity of submitting to it. They are strong enough in reputation, ability and virtue, to remodel their systems without impairing them, and to impose their own laws upon inferior rivals under the penalty of dissolution for non-obedience. Whether that example be followed or not, the student and the medical profession will be the gainers, and their own fame will be enhanced.

But what improvements should be made? These:—and whether adopted this year, or the next, or still later, beyond all peradventure there are those now in the profession who will live to see them all accomplished.

1st. There should be an examination of every student preliminary to matriculation, in which he should prove his acquaintance with the English language, or his native tongue, with the principal mechanical laws, and with the elements of inorganic chemistry.

2d. The annual term of Lectures should be extended to nine months, and not more than four lectures should be delivered on any one day.

3d. There should be a Professorship of Pathological Anatomy. The Lecturer on Chemistry should confine his attention to pharmaceutical and organic Chemistry. The Lecturer on Obstetrics should deliver a full course on the diseases of women and children. The Lecturers on Medicine and Surgery should treat of General Pathology in its application to their respective departments.

4th. There should be ample provision made for clinical (not amphitheatrical) instruction, but only for students in their second course.

5th. The examination for a degree should be public, and the candidate be obliged to *defend* his Thesis.

6th. No student should be permitted to receive a diploma who has not passed a satisfactory examination on ALL the branches in which he has been instructed.

These propositions are almost self-evident; but if there is any

one who denies or doubts them, the writer pledges himself to bring forward such proof of the evils they are intended to cure, as will silence denial and remove every doubt.

Will none of our institutions adopt them? Will not our Professors enter into a Holy Alliance to support one another in carrying them out? If they would preserve for our city its scientific reputation; if they would wither the weeds which infest the country and threaten to overshadow them; if they would be honoured and admired by the present generation, and gratefully remembered by posterity, they will heed this earnest voice, though it be neither the first nor the most emphatic that has sounded the alarm. But if they are deaf to its warning, or timidly refuse to place themselves in the van of the approaching revolution, it requires no prophetic vision to discover that the sentence has gone out against them,—that the *Mene Tekel* is written above their doors.

A. S.

Experiments on the Temperature of Bodies after Death. By Benjamin Hensley, Jr., M. D. (Communicated by Professor Dunglison.)

Marietta, Ohio, January 30, 1846.

My Dear Doctor:—Before leaving Philadelphia, I wrote an account of my experiments on *post mortem* heat, intending to leave it with a friend to hand to you; as I left the city quite unexpectedly. I notice this morning, for the first time, that this paper is in my trunk, and hasten to make amends by sending you a copy.

I used the thermometer mentioned by Dr. Dowler, of New Orleans, viz., Réaumur's, composed of a large glass cylinder, with the bulb at one extremity, the large cylinder enclosing a smaller one, and a graduated scale. With an atmospheric pressure of 30 inches of the barometer, the boiling and freezing points were found less than half a degree too low each, and I was assured by the maker that it was correct.

I give you a copy, verbatim, of my notes, that you may draw your own conclusions. I will further state that I made these experiments myself, and *know* they are scrupulously correct, so far as my agency is concerned. I cannot, of course, pretend to say they might not have been affected by manipulation and other causes, over which I had no control. In the notes below, the *time* the thermometer is retained in a region is first given, generally fifteen minutes (15′), and following this the number of degrees and minutes in Réaumur's scale.

The first case was an infant 10 months old. Three hours before death.—Temperature of room 19° R. Axilla, fifteen minutes (15′), twenty-three degrees (23°), thirty minutes (30′). Hand, 15′ 24° 30′. Fifty minutes after death.—Temperature of room 18°. Axilla, 15′ 23° 30′. Perineum, 25′ 22° 45′. Axilla, 25′ 23° 15′. Popliteus, 20′ 23°. Bend of arm, 20′ 22° 45′. Axilla, 20′ 22° 30′. Vagina, 15′ 21° 45′. Popliteus, 15′ 21° 45′. Bend of arm, 25′ 22° 30′. Perineum, 15′ 21° 15′. Axilla, 15′ 21° 45′. Vagina, 15′ 21°. Experiments closed.—Temperature of room 17° 30′.

Experiments resumed, 16 hours after death.—Temperature of room 17° 30′. Axilla, 15′ 17° 30′. Vagina, 15′ 16° 45′.

Nineteen hours after death.—Temperature of room 16°. Bend of arm, 15′ 17°. Axilla, 15′ 17°. Vagina, 15′ 16° 30′.

Thirty-six hours after death.—Temperature of room 14°. Axilla, 15′ 14°. Vagina, 15′ 13° 30′. Experiments now closed; signs of decomposition.

Case 2.—A man died of old age, (80 years.) 3 hours 40 minutes after death.—Temperature of dead-house 17° R. Axilla, 20′ 25°. Rectum, 15′ 27° 30′. Pharynx, 15′ 24° 15′. Popliteal region, 15′ 19° 30′. Bend of arm, 15′ 23°. Axilla, 15′ 23° 45′. Rectum, 15′ 26° 15′. Pharynx, 15′ 23° 30′. Bend of arm, 15′ 22°. Experiments now closed.—Temperature of room, 17°.

Eleven and a half hours after death.—Temperature of dead-house 13° 30′. Axilla, 15′ 21° 30′. Rectum, 15′ 22°. Bend of arm, 15′ 20°.

Thirty-one hours after death.—Temperature of room 16°. Axilla, 15′ 16°. Rectum, 15′ 16°. Experiments now closed: subject emaciated.

Case 3.—A man aged 40, died of cardiac disease, complicated with general dropsy.—Temperature of room 14°.

Thirteen hours after death.—Brain, 15′ 19° 15′. Axilla, 15′ 22° 30′. Scroto-inguinal region, 15′ 24° 30′. Between malleoli, 15′ 16°. Between left pleura, 15′ 24°. Thigh, (a deep incision,) 15′ 21° 45′. Brain, 15′ 18° 45′. Axilla, 15′ 21° 45′. Between left pleura, 15′ 23°. Scroto-inguinal region, 15′ 23° 15′. Thigh, 15′ 20° 45′. Between malleoli, 15′ 15° 15′. Scroto-inguinal region, 15′ 22° 45′. Experiments now closed.—Temperature of room 16°.

Thirty-six hours after death.—Temperature of dead-house 14°. Brain, 15′ 13° 45′. Between left pleura, 15′ 16° 15′. Scroto-inguinal region, 15′ 16°. Thigh, (old incision,) 15′ 14° 30′. Axilla, 15′ 16°. Between malleoli, 15′ 13° 30′. Experiments now closed.

This man is very much puffed up with water; decomposition has commenced; (fetor quite perceptible.) I notice local difference of temperature in the room; at the feet the thermometer stands at 13° 30′, while, up about the abdomen, it rises to full 14°.

Case 4.—A colored man, aged 50, died of diarrhœa. Temperature of dead-house 15° R.

Two and a half hours after death. Axilla, 15′ 27° 15′. Scroto-inguinal region, 15′ 26° 15′. Axilla, 15′ 27°. Between malleoli, 15′ 21°. Hand, 15′ 19° 15′. Brain, 15′ 24°. Between left pleuræ, 15′ 26°. Axilla, 15′ 25°. Thigh, 15′ 23°. Scroto-inguinal region, 15′ 24°. Temperature of room 15° 30′. Experiments now closed.

Nine hours after death.—Temperature of room 14° 30′. Brain, 15′ 20° 45′. Between left pleuræ, 15′ 25°. (In all cases the incisions were made valvular.) Thigh, (new incision,) 15′ 19° 30′. Between malleoli, 15′ 16° 30′. Scroto-inguinal region, 5′ 20° 45′. Axilla, 5′ 23°. (In my observations of the conduct of the thermometer, which were numerous, it always rose to the maximum in less than five minutes.)

Twenty-seven and a half hours after death.—Temperature of dead-house 14° 30′. Brain, (new opening,) 15′ 16° 30′. Between right pleuræ, (new opening,) 15′ 19° 45′. Scroto-inguinal region, 15′ 16° 45′. Malleolus, 15′ 14° 30′. Thigh, (new incision,) 15′ 15° 15′. Axilla, 15′ 18° 30′. Experiments now closed.

Case 5.—A boy, aged 16. One hour 45 minutes after death.—Temperature of dead-house 14° 30′. Axilla, 15′ 28°. Rectum, 15′ 30°. Popliteus, 15′ 24°. Axilla, 15′ 27° 30′. Bend of arm, 15′ 24°. Rectum, 15′ 29°. Brain, 15′ 24° 15′. Experiments now closed.

Seventeen and a half hours after death.—Temperature of room 14° 30′. Brain, (new opening,) 15′ 16°. Rectum, 15′ 19°. Axilla, 15′ 17° 45′. Taken away by friends.

Case 6.—A woman, aged 35, died of mania-a-potu. Ten minutes after death.—Temperature of room 15° R. Axilla, 15′ 27° 45′. Popliteus, 15′ 25° 45′. Removed to dead-house.

One hour and twenty minutes after death.—Temperature of dead-house 15°. Axilla, 15′ 25° 15′. Vagina, 15′ 29°. Experiments now closed.

Sixteen hours after death.—Temperature of dead-house 14° 30′. Axilla, 15′ 18°. Vagina, 15′ 19° 15′. Removed by friends.

Case 7.—A man, aged 36. Thirty minutes after death—Temperature of ward 18°. (This man died of ascites following prolonged intermittent. He was very much swollen. Indeed, the dropsy was general, as the whole subcutaneous cellular tissue was infiltrated.) The axilla gave in 15′ 27° 30′. Scroto-inguinal region, 15′ 28° 30′, (very much inflamed from dropsical infiltration.) Malleolus, 15′ 23° 15′. Removed to dead-house.

Thirty minutes afterwards.—Temperature of dead-house 15°. Axilla, 15′ 27°. Rectum, 15′ 28°. Thigh, 15′ 27°. Bend of arm, 15′ 24° 30′. Brain, 15′ 24°. Between malleoli, 15′ 20°. Scroto-inguinal region, 15′ 27°.

Seventeen hours fifteen minutes after death.—Temperature of dead-house 14° 30′. Scroto-inguinal region, 20′ 18° 15′. Brain, 15′ 15° 15′. Axilla, 15′ 19°. Rectum, 15′ 20° 30′.

Twenty-six hours after death.—Temperature of dead-house 17° 30′. Axilla, 15′ 17° 45′. Brain, 15′ 14° 30′. Rectum, 15′ 18°. Scroto-inguinal region, 15′ 16° 30′. Experiments closed.

Case 8.—A woman, aged 36, died of chronic dysentery; is emaciated.

Four hours and fifteen minutes before death.—Temperature of room 18° R. Axilla, 15′ 27° 45′. Hand, 15′ 24°. Popliteus, 15′ 27°. One minute post-mortem.—Temperature of ward 20°. Axilla, 15′ 27° 45′. Popliteus, 15′ 26°. Axilla, 15′ 27°.

Two hours and twenty minutes after death.—Temperature of dead-house 19° R. Brain, 15′ 24° 30′. Vagina, 15′ 26° 30′. Between left pleuræ, 15′ 25° 30′. Between diaphragm and liver, 15′ 25° 30′. Axilla, 15′ 23° 45′. Popliteus, 15′ 20° 30′.

Eighteen hours, post-mortem.—Temperature of dead-house 17°. Between right pleuræ, (new opening,) 15′ 17° 45′. Rectum, 15′ 18° 30′. Brain, (new opening,) 15′ 17°. Experiments closed.

Case 9.—A woman died of old age, (85 years;) very much emaciated.

Two hours after death.—Temperature of dead-house 18° R. Axilla, 15′ 28° 45′. Popliteus, 15′ 25° 30′. Axilla, 15′ 28° 15′.

Six hours after death.—Temperature of dead-house 19° R. Brain, 15′ 24°. Vagina, 15′ 28°. Between left pleuræ, 15′ 26°. Between liver and diaphragm, 15′ 26° 15′. Rectum, 15′ 27°. Experiments closed.

P. S.—I refer you particularly, in the above paper, to the difference of temperature in the brain and other regions.

I am, dear Doctor,
Truly yours,
Ben. Hensley, Jun.

Professor Dunglison.

On the Identity of Variola, Varioloid, and Variola Vaccina; and on the prophylactic power of the Vaccine Disease. By Jno. S. Rohrer, M. D.

Dr. R. M. Huston.

Dear Sir,—As the present epidemic of small-pox is attended with more than usual fatality, any information which may tend to the establishment of some facts at this particular juncture, may not prove uninteresting to the medical public. For this reason, I will submit to the readers of the Examiner some views which I have long entertained in regard to this important subject, and detail a few experiments in confirmation of the same, made as long ago as the years 1829 and 1830.

Very respectfully yours,
Jno. S. Rohrer.

Chestnut Street, Jan. 30th, 1846.

In the investigation of this subject, my attention will be chiefly directed to the establishment of the identity of the diseases mentioned in the title. But, in order to do this with effect, it will be necessary to inquire more particularly into the nature of varioloid. I will also make some observations, and offer one or two facts, in relation to the prophylactic power of the vaccine disease.

North America was not visited by the small-pox until the beginning of the seventeenth century. It made its first appearance in the colony of Virginia, and soon spread throughout the inhabited part of the North American continent.

From its history, the small-pox has always been regarded as a very formidable disease. Although subdued in part by the introduction of kine-pox, it is still considered one of the most destructive maladies in the catalogue of human diseases. In its varioloid form it is less malignant; but as it still possesses all the elements of small-pox in a modified form, it may, nevertheless, be considered as a serious if not dangerous complaint. During the last ten or fifteen years, this variety has at various times been more or less prevalent throughout the country. In the city and county of Lancaster, (Pennsylvania,) it was extremely common in the years 1829 and 1830. Indeed, more or less of it occurred in almost every family in that populous section. While engaged in this campaign, the majority of varioloid cases that came under my observation had previously undergone a course of the vaccine disease; but I never met with it in a subject that had been *recently and successfully vaccinated*. In a few cases only which came under my notice, the disease occurred

the third and fourth year after vaccination. This circumstance may be attributed to the vaccine disease not having been fully developed in the system, or to peculiarity of temperament. But, that the varioloid is small-pox modified by previous vaccination, or some other modifying influence, is a matter now too generally conceded to require much proof. If I were to adduce all the evidence in favor of this position, I might bring forward the most respectable authority; but it is unnecessary to go into this detail. There is one fact on this head which seems to be conclusive, and that is, matter taken from a varioloid pustule, and introduced into the arm of an unprotected person, will produce genuine small-pox.

The late Dr. Carpenter, of Lancaster, was one of the first who contended for the identity of the two affections; and although opposed by some of the most respectable physicians at that time, he nevertheless confirmed his position by experiments, which proved fully satisfactory. Having adopted the precautionary measures used in inoculation, the doctor introduced the matter of varioloid into the arms of two children who were unprotected; the result was *confluent small-pox*, in one instance of very aggravated symptoms, from which the patient finally recovered.

Another experiment, however, made by a medical friend of Dr. C., did not terminate so happily. The matter of varioloid (from a very mild case) was incautiously introduced into the system without preparing the patient for its reception, or restricting him in diet, &c.; and *confluent small-pox* was produced, of which the patient died.

These are a few of the many cases which came under the doctor's notice, in which the matter taken from a mild case of varioloid, unaccompanied by secondary fever, produced small-pox in patients unprotected. According to Dr. Eberle, the varioloid is not a new disease, but has been observed and described for centuries past, under the different names of *chicken-pox*, *horse-pox* and *swine-pox*.

Variola Vaccina.—In considering the subject of kine-pox, I shall endeavor, first, to give my views in regard to the origin and nature of the disease; secondly, the nature and extent of its prophylactic power.

With respect to the origin of this disease, different opinions have been entertained. Dr. Jenner believed that it originated from a disease in the heel of the horse, which farriers have termed the *grease*. Others have maintained that the disease in the cow was the only and true source of kine-pox. Dr. Jenner has, however, proved conclusively that the fluid of the grease,

when applied to the nipple or udder of the cow, was capable of exciting in that animal the kine-pox.

The experiments made by Dr. Loy, in order to ascertain the true origin of the cow-pox, leave no doubt that the matter of the cow-pox, and that of the grease, are of the same nature, and operate on the human system in the same manner. The result of his experiments would tend to confirm the accuracy of Dr. Jenner's investigations.

Having premised thus much, I will now venture to advance an opinion long entertained by myself, in regard to the origin of the vaccine disease, which is, that the *cow-pox* is the *variola* of the *human subject* in a modified form; and that it was originally excited in the cow in the form of *cow-pox*, and in the horse in the form of grease, by the direct application of *variolous contagion* to these animals.

The analogy between the variolous and vaccine pustules is so great, that Dr. Jenner himself was struck so forcibly by the resemblance, as to think it highly probable that the grease in the horse might be the source of small-pox. It is, therefore, a matter of no little surprise, that so profound an investigator as Dr. Jenner should not have conceived the idea of tracing it to the *variola of the human subject.* The doctor ingeniously demonstrated that the kine-pox was originally transferred from the cow to the human subject in the process of milking. Is it not equally probable that the *small-pox* was originally propagated to the cow and horse in the same manner? For example; the dairy maid being affected with small-pox pustules, incautiously bears her part in milking, and thus communicates the disease to the cow; in the same way the ostler infects the horse in the act of dressing the heel. The disease being thus transmitted to these animals, not only undergoes a great modification, but has also acquired a new character, and is no longer communicable from one individual to another through the medium of the atmosphere.

In support of the above views, while residing in the country, in 1829, I inoculated several cows with small-pox matter, but, owing to the imperfect insertion of the matter, on account of the refractory disposition of the animals, or, what is more probable, to the cows having previously had the kine-pox, I did not fully succeed. In one instance only the animal was affected with inflammation and slight tumefaction. But, in order to set my position in a more favourable light, I will adduce in support of this doctrine an experiment performed by the late Dr. Carpenter, of Lancaster. Several years ago the doctor introduced the variolous matter, by inoculation, into the system of the cow,

and the result, as he informed me, was a *perfect vaccine pustule.*

As it is an object of the greatest importance to ascertain the true source of the vaccine disease, I beg leave to suggest the propriety of *inoculating the cow with small-pox virus*, and permitting the matter to pass through different cows, until it shall be sufficiently modified to warrant the safety of its introduction into the human subject. Should this process be successful, as we have reason to hope, we can, at will, produce a modified disease which may prove the ultimate extinction of one of the most disastrous and deadly maladies that ever infested the human family!

If we reason from analogy, we are strongly supported in the opinion, that the diseases which we have been considering are essentially the same—apart from our present subject, the annals of medicine do not present an instance in which the influence of one disease, will secure the system from future attacks of another and a different disease. This anomaly, it would appear, is presented in vaccination alone. Should we not then be cautious in adopting a doctrine so much at variance with an established law? We should at least suspect it, until it had been brought to the test of repeated and close experiment.

In regard to the nature and extent of the prophylactic power of kine pox, I will now offer a few remarks, and adduce such facts as have been noticed by me during the late epidemic, as well as detail a number of experiments made in the years 1829 and 1830.

Dr. Jenner, as well as the most eminent physicians since his time, maintained the doctrine, that vaccination when successfully performed, is a permanent security against the infection of small pox. But, notwithstanding the high authority of Dr. Jenner, I do not believe that vaccination, however successfully performed, is a perfect security against future attacks of small pox, in all cases, unless the *operation of vaccination is from time to time resorted to.* That it wears out of the system is a fact which I believe can be fully and satisfactorily proven. Let us for a moment reflect upon the numerous cases which of late years have presented themselves to our notice, that had been successfully vaccinated, ten or fifteen years previously, and what has uniformly been the result? Why, that nearly the one-third of those who were exposed to the direct influence of variolous contagion, contracted the varioloid disease. In the years 1829 and 1830, while I resided in the city of Lancaster, the varioloid prevailed very extensively in that vicinity, so that very few families, as has been remarked, escaped the disease, although the precaution of vaccination throughout had been very strictly observed; but not in

one instance did I observe a case in which it followed *recent and successful vaccination.* It would appear therefore from observation, that the susceptibility to variolous infection is regained in a series of years. During the epidemic above referred to, I was engaged in vaccinating a great number of children, and the following are some of the experiments I tried:

Having procured a fresh scab, which bore all the characteristic features of the genuine vaccine crust, I introduced the infection into the arms of eight infants, not exceeding the age of one year, that were unprotected. Every one of them received the genuine disease, which ran through its regular course. The same infants were revaccinated at the expiration of nine months after the first insertion; but no other effect ensued than a slight inflammation produced by the point of the lancet. Matter was now taken from the *same scab* which *failed in producing effect in the eight infants*, and introduced into the arms of a number of children between the age of eight and ten years, all of whom were vaccinated within a year of their birth by regular physicians, and in each of these it produced a *small pustule*, with slight efflorescence round the base, and some febrile excitement. At the same time a number of persons between the age of fifteen and twenty-five were revaccinated from the *infection used in the former instance*, and nearly all were affected more or less with the vaccine disease. I have it from respectable authority that these persons had all been vaccinated in their first and second years, and from the description I received, they all passed regularly through the disease; the vaccine pustules, however, in the above cases were not equally developed; some arrived at the highest point on the fifth day, and some on the sixth and seventh, whilst others not until the eighth and ninth days after the impression. It was seldom that the vaccine pustule after revaccination continued to increase up to the ninth day, except in persons of more advanced age. In individuals between the age of fifteen and eighteen, the disease was observed to be very regular until about the sixth day, after which it would decline and leave a circular scab, with a slight depression in the centre. But in those persons between the age of nineteen and twenty to twenty-five, the disease approached nearer to the regular form; and in a few instances it approached so near, that little difference if any could be observed; except that the disease was developed between the seventh and eighth day from the insertion. In a great majority of individuals who had been vaccinated fifteen or twenty years previously, the pustule of the revaccination came to the greatest perfection about the *sixth day*, and had evidently a spurious appearance. The above experiments were tried and noted down in the year 1829; but I can confidently assert that I have since

revaccinated more than two hundred cases with similar results. In addition to the above, in the late epidemic, I revaccinated many old persons, and found, in the majority of cases, the disease approaching nearer to the genuine form than in younger ones.

From the foregoing facts and observations on this head, I think we may venture upon the following conclusion, viz: That all persons whose systems are in a state susceptible to taking the vaccine disease the second time, are also susceptible to the varioloid from the variolous contagion. I have never met or heard of a well authenticated case, in which the individual had taken the varioloid, after *recent and successful* vaccination; but hundreds and thousands have taken the disease ten, fifteen, and twenty years after the first impression of the kine pox. An infant will not take the regular vaccine disease more than *once in the same year.* We will venture to say that it is impossible to produce a second vaccine pustule in the same year. Thus we have a child secured against the varioloid influence in the first and second years after the impression of kine pox; but in eight or ten years more, its system is again susceptible to the influence of the variolous contagion. It does not seem that the vaccine disease does, in every instance, lose its prophylactic power; on the contrary, it is evident that in certain constitutions it affords complete protection against future attacks of the disease. But if the vaccine disease does not at all wear out of the system, to what should we attribute the frequent occurrence of the varioloid disease? It would be unreasonable to suppose that the affection affords complete protection forever after, and that the innumerable cases of varioloid which we daily witness, are owing to the carelessness or incompetency of physicians in performing the operation of vaccination.

The varioloid prevails most extensively in large cities, where vaccination is mostly performed by regular physicians. I attended on a family in the city of Lancaster, in this State, consisting of parents and eight children, all of which had received the regular vaccine disease, the latter in the first and second year of their ages, except the youngest child, which was unprotected, and at the age of two years took the small pox, and died on the eighth day. At the same time the father and the two eldest sons took the varioloid. The father had been vaccinated thirty years previously, and the two sons sixteen. All the rest of the family escaped the disease, although two of the younger children, between the age of four and six years, slept in the same bed in which the diseased was confined.

From these different experiments and observations, I have arrived at the following conclusions:

1st. That the *varioloid disease is small pox modified by previous vaccination, or some other modifying influence.*

2d. That the *cow pox is the variola of the human subject in a modified form; and that it was originally excited in the cow, in the form of cow pox, and in the horse in the form of the grease.*

3d. That the *kine pox is capable, in a series of years, of losing its prophylactic power.*

Case of Extreme Diffusion of Odours. By Alexander Wilcocks, M. D., of Philadelphia. (Communicated by Professor Dunglison.)

Dear Sir,—Your remarks last evening, upon the divisibility of odours, brought to my recollection a circumstance which I had nearly forgotten.

When at sea, two years ago, two hundred miles to the westward of the coast of Ireland, myself and many of the other passengers observed a smoky odour, which lasted during several successive days. An appeal was made to our captain for an explanation of the phenomenon. He informed us that he had frequently remarked it before, and that it was owing to the long continuance of easterly winds, which carried the odour of *burning peat* far out to sea.

Among the passengers was one gentleman, who, like myself, had lately listened to your lectures in the Jefferson Medical College. I need scarcely add, that the circumstance recalled to our minds the facts which you had adduced to prove this wonderful divisibility of odours.

A change in the wind, which soon occurred, carried our ship out of the latitude of the peat smoke.

With much respect, I am, dear sir,
Very truly yours,
Alex. Wilcocks.

Professor Robley Dunglison.

Philadelphia, 28th January, 1846.

BIBLIOGRAPHICAL NOTICES.

Medical and Physiological Problems, being chiefly Researches for correct Principles of Treatment in disputed points of Medical Practice. By WILLIAM GRIFFIN, M. D., Member of the Royal College of Surgeons in Edinburgh, one of the Physicians to the County of Limerick Infirmary, Consulting Physician to the Lying-in Hospital, &c.; and by DANIEL GRIFFIN, M. D., Member of the Royal College of Surgeons in London, Assistant Physician to the County of Limerick Infirmary, one of the Physicians to the Lying-in Hospital, &c. 8vo. pp. 359. London, 1845.

We rejoice to meet occasionally with a production from the press, which contains the results of observation and reflection on important topics appertaining to medicine. Yet we are rarely favoured. Since the "Notes" of Dr. Holland were published, we have seen none such. They were most favourably received by the thinking portion of the profession—alas! how few in number—yet they did not answer, in this country, the purposes of the publishers to the extent they ought to have done; and hence we have no new issue reprinted from the second London edition. This last we have the good fortune to possess, and know how to value it. The mass of purchasers look for what they consider the most *practical* works,—forgetting, that no correct practice can exist without a due scientific appreciation of every department of medicine. Were it otherwise, as well might we listen to the results of the empiric, who can *affirm* as strongly as, and more strongly than, the physician the success caused by the administration of his nostrums. *Principia non homines* was a motto—of very questionable latinity—adopted by one who wished to be regarded as the adopter of principles and not of men. It was wisely suggested, that *principia et homines* was more judicious; and so would we say of "facts not principles"—a motto absurdly chosen by a class of modern observers as an index of their rule of action. Let it be "*facts and principles;*" or rather "*principles and facts;*" for assuredly the establishment

of great principles to guide us in the management of disease is of infinitely greater importance than searching after isolated facts,—unless such search be undertaken by those who are eminently capable. It is proverbially difficult to trace the relations between cause and effect. It requires a mind well trained in psychical exercises; and yet we greedily devour, and implicitly credit, statements in the journals on evidence totally insufficient to convince a jury, or, indeed, a single individual whose mind has been properly disciplined in the search after truth. Let a formula for the cure of hemorrhoids be published, and instantly it is copied into every journal at home and abroad; whilst a careful and praiseworthy inquiry into the value of a medicinal agent, which leads to its being shorn of any reputed virtue, and to the beneficial results ascribed to it being transferred to great principles of hygiene, passes by unheeded.

The work before us consists of "principles *and* facts;" and the very first paper is a good specimen of the whole. It is entitled "*Inflammation of the Bowels treated by Opium.*" We hail its appearance with pleasure, because it confirms the view, which we have always embraced, that opium, in *proper doses*, is one of our most valuable agents in inflammatory affections. When given in small quantities, it is an undoubted excitant. The error has been to regard it as such when administered in large doses. It is then unquestionably a sedative, and one of the best that we possess. In painful inflammatory and other affections, its influence is most precious. Not only does it positively diminish the activity of the organic actions; but it blunts the sensibility of the nervous system, and prevents the irritation that would necessarily arise from nervous irradiations in painful disorders, which tend so much to aggravate or complicate the original malady.

In inflammation of the bowels its use has been thought to be contraindicated, under the belief, that it has a constipating effect, and may therefore augment the mischief in sero-enteritis; yet such is by no means the fact. By reducing the inflammation, the action of the bowels will be restored; and if opium be capable of accomplishing this, then its effects are rather laxative than constipating. In endo-enteritis, however, or inflammation of the mucous coat, this objection to opium—if well founded—could not

apply, for in it there is increased secretion from the lining membrane. Principles and facts have proved to us, after a long experience, that it is equally suited for both these pathological conditions. It is, we repeat, in appropriate doses, a precious sedative,—and therefore adapted for the removal of inflammation—mucous as well as serous. On this point we wish to speak emphatically; but then the remedy must be given freely. Our own course is to prescribe it in soft pill,—not less than two and a half or three grains; and if, in the course of four hours, the symptoms do not yield, to repeat the dose. Commonly we premise a full bleeding from the arm, and follow this immediately by the opium. The value of the combination is now almost universally admitted; and let the observation of Dr. Armstrong—acknowledged to have been a most painstaking and excellent practitioner—be borne in mind—that if in enteritis he were told, that he must rest his hopes of safety on the lancet or on opium, he would unhesitatingly prefer the latter. "So great," says he, "is my confidence in full doses of opium in peritoneal enteritis, that if compelled to say, and supposing myself the subject of the disorder, whether I would exclusively rely upon them solely, or upon bloodletting solely, I should certainly fix upon the former; at the same time I should like to have the simultaneous influence of both remedies, being convinced they are by far more serviceable combinedly, than separately employed."

It has been already remarked, that opium, by removing the enteric inflammation, may restore the alvine evacuations, which have been withheld owing to such inflammation. It has been a question, however, whether cathartics should not be given also. It need scarcely be said, that where parts are inflamed, anything that is calculated to irritate them should be carefully withheld. No one would doubt the truth of this as a general principle, nor can we readily imagine the special case that should form an exception. Yet by the routinist, cathartics have been advised to meet a mere epiphænomenon; for the constipation, in almost all cases of sero-enteritis, is the consequence rather than the cause of the phlegmasia.

It was a wise remark of Dr. William Saunders, that the best agent for opening the bowels in enteritis is the lancet; and upon the same principle we would urge opium, alone or in conjunction

with the lancet; but it may admit of serious doubt, whether any active cathartic should ever be given. Dr. William Griffin, who is the author of Problem 1,—*What principles should be kept in view by the physician in the treatment of enteritis,*"—does not believe purgatives to be at all times unadvisable in enteritis.

"There are," he says, "occasions in which they are of use, even in the early stage; but it is difficult to offer indications by which we shall recognize these occasions in practice. In the advance of the disease, when its force is considerably broken, and the bowels may be supposed capable of acting without increasing or renewing the inflammation, there must be an obvious advantage in getting rid of the contents of the bowels, and this may perhaps be then generally effected with safety, by means of mild purgatives, combined with henbane. If, however, there was no injurious distension present, and the inflammation was progressively declining, my disposition would be to await a more perfect amendment before I would give even these."

And he subsequently adds:—

"General experience testifies, that the strongest purgatives will not operate in the early stages of inflamed bowels, unless large depletion by the lancet has been premised,—that is, unless the violence of the inflammation has been in some measure subdued: while on the other hand, as soon as this has been accomplished, they commonly occur spontaneously, or with the assistance of the mildest purgatives.

"Notwithstanding the free operation of purgatives at an early stage of enteritis, the inflammation may proceed to a fatal termination unless arrested by other remedies.

"A purgative has been known to occasion inflammation of the bowels, and when inflammation has been subdued by other remedies, it has brought on a recurrence of it.

"Inflammation of the bowels may be perfectly subdued without any evacuation at all.

"The bowels may even sometimes continue in a confined state for three or four days after the inflammation has subsided, without occasioning injurious distension." p. 34.

All this is discriminating; and experience of some length leads us to declare, that it perfectly accords with "principles and facts!"

The next problem, by the same gentlemen, is—*How are nervous affections distinguishable from inflammatory?*" Dr.

William Griffin has been long known for his views on spinal irritation, of the existence of which nothing either he or any one else has hitherto written, has carried conviction to our mind. Nor is the present paper more convincing. His view on this important topic of differential diagnosis is the following:

"In all acute inflammations of vital organs, I believe that no spinal tenderness will be found, except where it existed previous to the supervention of the attack," [he adds in a note, "some writers on spinal irritation state that they have found spinal tenderness with inflammation of liver,"] "or where the spinal cord itself happens to be the seat of such inflammation. In all neuralgic affections, on the contrary, tenderness of some portion of the spinal column, *usually that corresponding to the affected organ*, may be detected, except in some rare cases in which it seems probable the ganglionic nerves alone are concerned. As these cases must still present a difficulty in their diagnosis, we must rest contented with those general characteristics, which, however vague, or liable to lead us into error, are all we have to guide us, and all we have hitherto had to determine our opinion in that large class of neuralgic affections, for the detection of which I have been offering a new, and I believe, less doubtful sign."

Spinal irritation is a hobby with the author; and one that he has ridden somewhat unmercifully, with spur and whip. We are not surprised, consequently, at his attaching undue importance to it. No one, not similarly prepossessed, could have ventured to lay down so inadequate a test in doubtful cases; and we question much, whether Dr. Griffin himself is not rather guided in his judgment by more rational concomitant phenomena. In this case we want from him both "principles and facts."

Problem third, by the same author,—"*what is the diagnosis of abdominal inflammations?*"—is an embodiment of the same notions that are comprised in Problem second; whilst Problem fourth gives his views at length on "*spinal irritation*"—on which we do not think it worth while to dwell—until the existence of the malady has been proved. As yet it may be esteemed wholly hypothetical.

The next problem—the fifth in the series—by Dr. D. Griffin, inquires "*under what circumstances, and to what extent, is bleeding proper in diseases of the brain?*"

The Author concludes by recapitulating the circumstances in which he considers large bleedings improper, with the principles on which, in his estimation, they are so.

"*First.* They are improper, because symptoms are no certain test of the amount of disease, and we may produce a degree of debility that cannot be contended with.

"*Secondly.* They are improper in all cases in which extensive disease of brain is known or suspected to exist, as in such cases, besides the immediate danger, they produce a degree of debility that would interfere with the process of reparation, so far as that is possible.

"*Thirdly.* They are improper in all cases of disease of brain attended with severe and protracted pain, as such cases usually die, not from any mechanical effect of an existing inflammation, but from the exhaustion produced by the pain that accompanies t.

"As a general rule, I would, therefore, even in circumstances in which the loss of a large quantity of blood was supposed to be necessary, prefer taking it away in moderate quantities at certain intervals, watching the progress of the case, and being guided entirely by it." p. 87.

Problem VI, by Dr. William Griffin, is "*On what morbid state does the occurrence of Coma and sudden death in Jaundice depend?*" In this we see nothing new. The author considers it to be superinduced by the "suppression of a most important excretion, as it sometimes is in the suppression of the catamenia, and almost always of the urine. When we find inflammation of the brain or its membranes suddenly brought on by the obstruction of the uterine or renal discharges, we cannot be surprised that a suppression of one of the most important in the whole system, whether as a secretion or an excretion, should occasionally be induced." p. 96.

In Problem VII, "*Is the law of visible direction, as at present received, a true one?*" Dr. D. Griffin objects—and we have long maintained the same opinion—that Sir David Brewster's views on the law of visible direction are not borne out by facts. It is now, indeed, passed over almost *sub silentio* by some of the best writers on optics.

An elaborate inquiry in Problem VIII, "*Is Laryngismus stri-*

dulus, or the crowing disease, a spasmodic or paralytic affection?" leads Dr. Wm. Griffin to the following summary of the amount of our present information, and of the facts connected with the disease.

"By the concurrent testimony of almost all who have noticed the affection, it occurs, for the most part, if not wholly, in strumous habits.

"It is frequently found in connexion with enlarged glands in the neck, and perhaps in the thorax.

"It is frequently found in connexion with eruptions on the face, ears or scalp.

"It frequently terminates in convulsions, and is sometimes, though very rarely, ushered in by them.

"It is met with in families in which children are subject to head affections or convulsions, but who have also the strumous disposition.

"It is sometimes met with in connexion with an apoplectic or comatose state from the commencement, as in the cases of crowing apoplexy, which I have described.

"In a great proportion of the cases which terminated fatally, there was no symptom of head affection through their whole course, beyond the occasional fits of breathlessness and crowing: and then the children were as well, apparently, a few moments before death, as they were previous to the first attack of the disease, or as any children could be.

"The complaint is sometimes, but rarely, attended by cough or permanent difficulty of respiration.

"I believe it may be said that from one-third to half of all the cases of which we have any account, terminated in death. But this great mortality may perhaps in some degree be attributable to the over-active treatment pursued in many instances, from mistaken notions of the congestive or cerebral nature of the disease." p. 140.

We pass over Problem IX, by Dr. W. Griffin, "*Does suffering necessarily imply self-consciousness? Are sentient beings necessarily percepient?*" which is an elaborate physiological inquiry,—and Problem X, by the same writer—"*What are the therapeutic effects of opium?*"—on which the author's views are sound, and well worthy of perusal; and proceed to Problem XI, also by Dr. W. Griffin, "*What principles should regulate the treatment of Hæmoptysis?*" One great object of the author is to deprecate the too constant employment of frequent depletion and protracted

low regimen. He properly remarks, that in most cases hæmoptysis occurs in constitutions predisposed to tubercular phthisis, "and it is equally well known, *that in such constitutions tubercles are readily generated by any deterioration of the vital power.*" Another weighty objection, however, is, that repeated blood-letting communicates a tendency to the recurrence of the hemorrhage, especially if fluids be freely allowed. Loss of blood augments absorption; fluid readily passes into the vessels by imbibition; the blood is thus rendered thinner, and better adapted for escape by diapedesis.

In Problem XII, "*How should acute rheumatism be treated?*" Dr. W. Griffin discusses the opium and calomel treatment of Dr Hope, and the opium treatment of Dr. Corrigan. The latter prescribes opium in doses of one or two grains every second or third hour, or of ten or twelve grains or more in the twenty-four hours. It is not improbable, that the entire efficacy of Dr. Hope's plan is dependent upon the opium. "The most important rule to be remembered," says Dr. Corrigan, "in employing opium for the cure of acute rheumatism is, that *full and sufficient doses* shall be exhibited. I have heard of opiate treatment having disappointed those who have tried it. On inquiry, I have learned that in those cases it has been given only to the extent of a grain every fourth or sixth hour. This is not the treatment of rheumatism by opium; it is making the patient worse than before; it is inflicting on the patient the mischief arising from the stimulant effects of the drug, and withholding from him all the benefits of its sedative influence. The opium should always be increased in dose, both as to frequency and quantity, until the patient feels decided relief; and should be then kept up at that dose until the complaint is steadily declining."

The average time of cure under Dr. Corrigan's treatment is nine days. We can speak, from positive experience, of the marked value of opium in this affection. A case has just passed from our hands to a state of health, where not more than six grains were given in the course of the day; yet all the arthritic phenomena had disappeared by the end of the week.

The last paper is the joint production of the two Griffins. It

is entitled "*Observations on the application of Mathematics to the Science of Medicine,*" in which the importance of medical statistics is properly urged,—but with no new views. To therapeutics it is confessedly difficult of application, and this very difficulty will prevent it from being had recourse to by the mass of the profession.

Such are the topics investigated in the work before us:—all of great interest; and some of deep moment. Excepting where the authors are carried away by preconceived notions—as in the case of spinal irritation—their views are generally sound, and manifestly the offspring of reflecting minds: and if they do not always add to our stock of knowledge, they are eminently suggestive, and may lead to farther investigation.

A Manual of the Diseases of the Eye. By S. Littell, M. D. One of the Surgeons of the Wills' Hospital; Fellow of the College of Physicians of Philadelphia, &c. &c. Second edition. Revised and Enlarged.

The first edition of Dr. Littell's Manual was published in 1837, and contained 250 pages. Its favorable reception was attested by its republication in London, under the auspices of Dr. Houston, a distinction not often awarded to American works, notwithstanding our avidity in seizing upon the books of our transatlantic brethren, and dressing them up to suit the home market—a practice, by the by, rather derogatory to native talent, and calculated, we fear, to retard the advancement of the medical literature of our own country. The present volume is got up in a style quite superior to its predecessor, besides being enlarged to the dimensions of 372 pages.

The chief merit of the work of Dr. Littell is its conciseness and perspicuity—embracing, as it does, a description of all the important diseases of the eye and its appendages—without the multiplicity of names, the useless distinctions, and the elaborate and verbose disquisitions which distinguish the productions of some foreign writers on Ophthalmology.

The author's connection with the Wills' Hospital for the lame

and blind, has given him opportunities for observation and study in this department of surgery which few can enjoy; and we are happy to find that he has frequently introduced into his Manual the results of the experience of himself and colleagues upon important questions of ophthalmic practice. In treating of the operation for cataract, we find the following instructive remarks, which will give the reader an idea of the author's style, and of the practical tendency of his work.

"The operation is performed in three different modes,—extraction, depression, and division or absorption,—each of which possesses advantages which may render it eligible under certain circumstances.

In this country the operation by *absorption* is generally preferred; it is well adapted to the removal of soft or fluid cataract, the usual form in which the disease appears in early or middle life; requires little manual dexterity; is attended with less danger to the eye than either of the others; and may be employed in a multitude of cases where, from complications and other causes, extraction is inapplicable. The principle on which it is founded, is the removal of the lens by the agency of the aqueous humour, admitted through an opening in the capsule. The pupil having been previously dilated by the application of belladonna to the brow, and the patient placed upon a table in a supine position, supported by pillows, an assistant retracts one of the lids, while the surgeon takes charge of the other, and introduces a sharp-edged needle,—either straight or very slightly curved, and about eight or ten lines long in its shaft,—through the sclerotica, parallel with its fibres, a little above its transverse diameter, and not more than a line from the cornea. The point of the instrument having been directed towards the centre of the eye while piercing the sclerotica, should be brought forward as soon as it has fairly penetrated that membrane, by depressing the handle towards the temple, and after being rotated one-fourth of a revolution, so as to bring its flat surface upwards, carried between the fringed extremity of the ciliary processes and the circumference of the capsule, across the posterior chamber, a little beyond the middle of the pupil. If the needle have penetrated the eye too deeply in the first instance, it should be partially withdrawn, else it would be entangled in the substance of the lens; and, while endeavouring to avoid this accident on the one hand, some care is necessary not to injure the iris on the other. When the instrument, thus dexterously guided through the narrow passage, has fairly reached the centre of the pupil, its cutting edge is turned towards the opacity, and the capsule with the

crystalline gently divided into several pieces. In performing this part of the operation, it is advisable to raise the needle repeatedly clear of the cataract, and re-enter it again in a different part, but not to raise the point too near its superior margin, otherwise it would be liable to be dislocated, and revolve upon its axis;—a circumstance, however, from which no great mischief would arise, except, perhaps, a greater tendency to inflammation from the pressure upon the iris. If the lens happen to be fluid, it escapes immediately on opening the capsule, and is diffused through the aqueous humour; and when it is of firmer consistence, small fragments are often observed floating through the chambers.

In those instances where the cataract is soft or fluid, a single operation is sufficient for its removal; but under other circumstances it is often necessary to repeat it after an interval of six or eight weeks. On these subsequent occasions, the crystalline should be more completely comminuted, and the fragments passed into the anterior chamber, where absorption is supposed to take place more rapidly;—care being taken to avoid, if possible, the posterior hemisphere of the capsule.

The mode of operation just described, is that usually adopted at the Wills Hospital, both by the author and his colleagues, and has been followed by a remarkable degree of success; the consequence, probably, of the great attention which is given, both in the preparatory and subsequent treatment, to the prevention of inflammation."

There is another point in which the work of Dr. Littell differs widely from some of the late publications on ophthalmology, and which, in our judgment, forms one of its strongest recommendations. Undue prominence is not given to the description of operative procedures upon the eye, to the neglect of the more ordinary diseases of the organ which are constantly occurring to the practical physician, and about which he desires to be informed. Long descriptions of the various methods of operating adopted by different surgeons, with plates of instruments, &c., many of which are never brought into use, may serve to amuse the eye, and swell out the pages of a book, but they can have no practical benefit. The late popular operation of strabismus, for instance, which has occupied so much space in some of the recent works on diseases of the eye, is disposed of by our author in four and a half pages; and we must be allowed to say that, to our mind, he has in this short space presented a more philo-

sophical view of the nature of this deformity, and of its means of cure, than is contained in some of the long treatises to which we have referred. It is true that our author does not undertake to enter into detail upon this or any other subject, in what professes to be a "Manual," while, at the same time, he has generalized so judiciously, that his "little volume" will, we think, be found quite as instructive, both to practitioners and students, as many of the more voluminous works on the same subjects.

We have long believed that one cause of the ignorance which prevails in the great mass of the profession, upon this department of surgery, arises from the forbidding aspect of the books which treat specially of it. It is looked upon as a sort of insurmountable speciality, which requires long and arduous study, to master all its details. Its terminology alone excites alarm, and warns the student of its approach, and hence he too often abandons it in despair. If the work of Dr. Littell should tend to divest the subject of these difficulties, and to present it in a more simple and attractive form, he will have performed a service to the profession by no means unimportant.

It is, therefore, with great pleasure, that we recommend the volume before us, both to the practical physician and the student; and although it may not take the place of the larger works of Lawrence, Mackenzie, Middlemore, Morgan, and others, it is yet an admirable treatise, which does great credit to the author, and cannot but prove an acceptable offering to the profession.

P.

General Therapeutics and Materia Medica, with one hundred and twenty illustrations. Adapted for a Medical Text Book. By Robley Dunglison, M. D., Professor of Institutes of Medicine, etc., in Jefferson Medical College of Philadelphia, formerly Professor of Materia Medica and Therapeutics, etc. etc. *Third edition, revised and improved. In two volumes,* 8vo. Lea & Blanchard: Philadelphia, 1846.

In these times of rampant empiricism and bold pretence, when so many commend themselves to favour by decrying all theory as idle speculation, and rejecting every thing but experience, and all experience but their own, it is encouraging to find a work

devoted to the inculcation of principles in medicine so well sustained by the profession as the present. The short time which has elapsed since the publication of the former edition of this standard work, sufficiently testifies that there are still among us many who appreciate science, and are not yet disposed to worship the Baals of the day. Facts and principles must ever elucidate and sustain each other. No one is silly enough to contend for principles that are inconsistent or in direct opposition to well established facts—but facts must not be confounded with assumptions—nor the limited observations of individuals received as truths, which contradict the testimony of thousands who are equally competent to observe and impartial to declare. It is on this rock that so many split. In their eagerness to avoid unfounded hypothesis, they plunge into the blindest empiricism. In the appropriate language of our author:—

"A knowledge of the healthy and diseased functions, or of physiology and pathology, and of the ordinary effects of therapeutical agents on those functions, obtained by careful and repeated observation, must be the basis of that enlightened theory, which necessarily leads to enlightened practice; and great mischief would result to both, were we to discard all rational therapeutics, and restrict ourselves to mere observation. The complex functions, executed by the human organism, are so modified by multitudinous external and internal influences, which are inappreciable; so much agency is perpetually exerted by the *moral* over the *physique*, that no comparable facts can be obtained in sufficient number to admit of any accurate numerical deduction; and constantly, we must either treat disease in accordance with principles suggested by conjoined observation and reason; experiment for ourselves *ab initio;* or resign our faith to the asserted observation and experience of others; and of these, which of the legion shall we select as masters? It is fortunate that we are possessed of such principles in medicine. Without them we should be unable to meet morbid manifestations, which present themselves to us for the first time. 'He,'—says Dr. Abercrombie,—'who follows certain arts or practical rules, without a knowledge of the science on which they are founded, is the mere artizan or the empiric; he cannot advance beyond the practice-rules which are given him, or provide for new occurrences and unforeseen difficulties.

"These great principles are the same everywhere, and by their possession we can combat disease wherever we meet with it; amongst the equatorial heats, or the Siberian snows; in the scorching presidencies of British India, and *à fortiori* in every

portion of this wide-spread territory; the lofty mountain, and the lowly valley; the pestiferous locality on the banks of the Mississippi, and the more salubrious regions where malarious influence is unknown. It is by their possession, that the medical officers of the army and navy know how to manage the diseases of all climes, when opportunity is offered them for adequate observation. That diseases are modified by climate or locality cannot be doubted, but the well instructed physician speedily seizes hold of the peculiarity."

The therapeutic principles advocated in this edition, are substantially the same as presented in the former, and constitute, in our estimation, the chief value of the work. We have had too many occasions for deploring the neglect, by otherwise well informed practitioners, of a proper attention to this subject, not to feel anxious that works of this character should be generally studied. Assuredly, nothing so much contributes to the multiplication and support of quackery, in its various phases, as the empirical practice of honorable members of the profession. By empirical practice we mean prescribing articles as specifics, or cures, for disease, whether one or many compose the prescription, without duly considering the pathology, and well understanding the physiological actions invoked or excited by the remedies employed—without, in short, a proper comprehension of what is to be expected of the natural offices of the system, and the kind and degree of control or assistance afforded by the treatment pursued. The pragmatical disposition to interfere on all occasions of disease,—to regard nature as always wrong,—that every thing is to be done by physic and nothing left to the recuperative powers of the system,—is the greatest fallacy prevailing among those of our art. Too little attention to hygiene and too much reliance upon perturbating remedies, has, we verily believe, done more towards weakening the confidence of the public in our noble profession, and strengthening the hands of Charlatans, than all the lying and trickery of these detestable knaves, favoured as they are by the weakness and credulity of their dupes.

The extensive circulation of the two former editions of this work of our author, and the manner in which its doctrines have been noticed by the leading journals of this country and Europe, renders it unnecessary for us to speak of the different parts of the

work, or indeed of any part in detail. We cannot close these remarks, however, without expressing our great satisfaction with the highly practical views of the author on the subjects of bloodletting and the use of opium in the treatment of inflammation. The error of resorting to bloodletting in all inflammations, or of relying upon it exclusively, or nearly so, in any, as some do, is well set forth; as well as the unfounded prejudice of many against opium in conjunction with bloodletting, in inflammations involving important organs.

The Materia Medica portion of the work contains the usual amount and kind of information on that subject. All the principal articles and means of cure are treated of, briefly, but with sufficient detail for the purposes of either students or physicians. Scattered through the volumes, we have numerous wood-cut illustrations of medicinal plants, mostly well executed, which add to the value of the publication.

The Natural History and Diseases of the Human Teeth. By Joseph Fox, M. R. C. S. L., &c. First American from the third London Edition. Remodeled, with an introduction and numerous additions. By Chapin A. Harris, M. D., D. D. S., Professor of Practical Dentistry and Dental Pathology in the Baltimore College of Dental Surgery, etc. etc. Illustrated with Thirty Plates. Royal 8vo. pp. 440. Ed. Barrington & Geo. D. Haswell. Philadelphia. 1846.

Mr. Fox was originally a pupil of the celebrated surgeon, Mr. Cline, and served under him as a dresser in Guy's Hospital. Circumstances afterwards induced him to devote much of his time to the study of the teeth and their diseases; on which subjects he subsequently became a distinguished lecturer at Guy's Hospital. The first complete edition of his work, on the history and diseases of the human teeth, was published in 1806, and has continued to enjoy the highest reputation ever since. No work on the subject has ever appeared for which intelligent dentists have manifested a higher regard. The present edition comes forth with the advantages of a careful revision, and the sanction of a distinguished American teacher and practitioner of the dental art.

In preparing the work for republication in this country, the editor informs us that he has found it necessary to make numerous and extensive additions, in order to adapt it to the present state of dental practice. That these additions are judicious, we need no better guarantee than the known competency of the editor.

The mechanical execution of the present edition is in the highest degree creditable to the enterprising publishers.

THE MEDICAL EXAMINER.

PHILADELPHIA, MARCH, 1846.

HEALTH OF PHILADELPHIA.

The inhabitants of the city of Philadelphia probably never enjoyed better health in the month of February than the present year. The small-pox, which has been more than usually prevalent, is rapidly disappearing, and we have reason to believe will shortly cease to exist. This arises, doubtless, from exhaustion of the materials for its continuance, in consequence of those subject to it having either had the disease or been vaccinated. Although there have been numerous instances of its occurrence in persons who had been vaccinated, as well as in some who had already had it by inoculation, and in the natural way, we believe we state the universal conviction of the profession when we assert, that the value of vaccination was never more triumphantly manifested than during the recent epidemic.

TRANSYLVANIA UNIVERSITY.

By a slip from the Lexington Observer, we are informed that Dr. L. G. Watson has resigned the Chair of Theory and Practice of

Medicine in this Institution, and that Dr. Bartlett, the former occupant, has been appointed to fill the vacancy. Dr. Bartlett is now in Europe, but no doubt is entertained of his acceptance of the place.

Dr. Samuel Anan, of Baltimore, has been elected to the Chair of Obstetrics in the same school, vacated by the death of the late Professor Richardson.

On the resignation of Professor Watson, the class held a meeting, at which resolutions were passed, expressing, in appropriate language, their regret for the condition of the Professor's health, which rendered it necessary for him to retire, and their respect for him as a man and a teacher.

NEW MEDICAL SCHOOL.

The Legislature of Pennsylvania has recently chartered a new Medical College, to be located in Philadelphia, and called the "Franklin Medical College," of which Drs. Goddard, Clymer, Tucker, Rogers, Biddle, Van Wyck and Joynes, are to compose the Faculty.

NEW PUBLICATIONS.

Since the publication of our last number, in addition to those noticed in the present, we have received the following valuable works, which we shall notice more particularly hereafter, viz.: Velpeau's Operative Surgery, by Prof. Mott; Lectures on the Operations of Surgery, by Liston and Mütter; Liston's Elements of Surgery, with notes and additions, by Prof. Gross; Compendium of Lectures on the Theory and Practice of Medicine, delivered by Prof. Chapman, by Benedict; A Manual of Chemistry, by Hoblyn; Examinations in Anatomy, Physiology, Practice of Medicine, &c., by Hooper; Lecture on Medical Obedience, introductory to the course of Theory and Practice of Medicine in the Medical Department of Pennsylvania College, by William Darrach, M. D., etc.

ANNUAL REPORTS.

We have received the "*Twenty-fifth Annual Report of the Bloomingdale Asylum for the Insane. For the year* 1845." From Dr. P. Earl; and also,

The "Report of the Pennsylvania Hospital for the Insane, for the year 1845." From Dr. T. S. Kirkbride.

We have not had time to look into these "Reports," but feel assured that they lack none of the interest which characterized their predecessors. Will not some of our Correspondents furnish us with an analysis of these and others from kindred Institutions?

CATALOGUES.

We have before us the following Catalogues of Medical Students the present year:

"Western Reserve College,"—number of Medical Students,				160
"University of Pennsylvania,"	"	"	"	462
"Jefferson Medical College,"	"	"	"	469

RECORD OF MEDICAL SCIENCE.

Report on the Progress of Anatomy and Physiology.

By W. H. Ranking, M. D.

§ VII.—*Generation.*

34. *Analogy of menstruation with the "rut" of Animals, &c.*—Few subjects have had so much attention directed towards them, or have been so fully investigated of late years, as those which have reference to the phenomena of menstruation, the maturation and discharge of ova, and the formation of corpora lutea. All have received considerable elucidation, and the result has been that many highly interesting and important points have been ascertained concerning each, and what was before considered strange and anomalous, is now shown to be all order and harmony. Thus menstruation, which until of late was almost universally regarded as peculiar to the human female, is now proved beyond doubt to be analogous in all essential particulars to the phenomena attending the process of *heat* or *rut* in animals. The investigation of many British and continental physiologists have established the truth of this analogy almost beyond question. Among the most recent labourers on the subject of menstruation is Mr. Girdwood, who, in a capital paper, has summed together most of the facts establishing the above-mentioned analogy, and has,

moreover, strengthened these facts by the results of several original experiments. He draws attention to the following conclusions, (the first two of which, however, have been also well established by the researches of Bischoff, Raciborski, and other recent inquirers.) 1st. The catamenia appear in lower animals as well as in the human female, and whilst the discharge is characterized in them as in her, by a periodicity peculiar to each separate genus, it is (at least in the higher orders of mammals) equally sanguineous in them as it is in woman. From observations on the dog, rabbit, cow, and mare, Mr. Girdwood proves that the occurrence of a periodic discharge in them attends the phenomena of heat, and the discharge microscopically and chemically bears a more or less close resemblance to that of the human female, and he observes that in passing up through the series of animated beings, it would appear that the nearer we approach man, the more the catamenial discharge approximates in character that of our own species. It appears in all to consist of a profuse periodic flow of the usual mucous secretion of the organs of generation, with, in the higher genera of animals, the super-addition, generally, of more or less blood diffused in the secretion, and from this its diffusion deprived of its usual amount of coagulation. 2d. In woman, and in the females of all animals in which the periodic discharge is apparent, the discharge is indicative of the maturation of an ovum, and of its being on the point of elimination from the ovary ; and the capability for impregnation is, during menstruation, at its acme. This corresponds with the observations of Bischoff, Pouchet, Raciborski, &c., and is now a generally adopted principle ; for although, on account of the minute size of the human ovum and the rare opportunities afforded of examining the bodies of women who have died during the menstrual period, it is difficult to prove the question beyond doubt by the discovery of ova, yet sufficient observations have been made to render it pretty certain that the above observation is correct, so far as the human female is concerned. With regard to animals, numerous observations and experiments, especially those by Bischoff and Raciborski have rendered it quite certain that at every period of heat one or more ova are matured and discharged from the ovary, and this moreover quite independent of sexual intercourse. The statements of these eminent physiologists are founded on the surest of all evidence, the actual detection of ova in the fallopian tubes of animals in whom sexual intercourse had not taken place ; thus Bischoff found in a lamb, killed a few hours after becoming in rut for the first time, (and in whom coition had not been effected,) a ruptured Graafian vesicle in the right ovary, and an ovum in the corresponding fallopian tube. Again, in a bitch two days after becoming in heat, and apparently inclined to admit the male, (which it was prevented from doing,) Bischoff extirpated the left ovary and fallopian tube, and closed the wound by suture ; no Graafian vesicle in this ovary had burst, but four were extremely turgid. Five days afterwards the bitch was killed ; four large corpora lutea were found in the right ovary, and four ova in the corresponding fallopian tube.

From this it appears quite certain, as Bischoff concludes, that during the rut of animals, ova become detached from the ovary and enter the fallopian tube where they perish, unless sexual intercourse coincidly occurs, in which case the ova in all probability become impregnated. The same may be considered to hold good in the human female. Premature appearance of the catamenia seems to depend, according to Raciborski, on the precocious developement of the ova; a circumstance in favour of this opinion is the invariable coincidence of developement of the mammæ and external genital organs with the occurrence of the hemorrhage, in cases of premature menstruation. There are still wanting observations to show what is the exact purpose served by the periodical discharge of a sanguineous fluid attending the maturation and discharge of ova; numerous ingenious suggestions have been made, but the right one does not seem yet to have been hit upon. There seems no doubt but that all the phenomena of menstruation are primarily dependent on the ovaries, which act sympathetically on the uterus, as well as on other organs. A main argument which has been advanced against menstruation in the human female being a process identical with the rut of animals, is the disinclination to sexual intercourse evinced by woman during the menstrual period, whilst it is only during the period of rut that animals will admit the male; but this Bischoff (as also Mr. Girdwood) considers to be the result of habit and the natural delicacy of the sex, rather than of actual disrelish; and it is most probable that in woman as well as in the females of animals, the desire for sexual intercourse is greatest at the menstrual period or heat, especially towards the decline of the discharge, at which latter period, from observations on animals, it is proved that the ova are usually discharged; bitches are generally observed to be languid and to refuse the male during the first few days of heat, but after thisthey become lively and readily admit of being lined; so, analogous to this is the ailment of the human female during the early part of each menstrual period, and previous to the discharge becoming fully established. 3. A third conclusion arrived at by Mr. Girdwood, in the above-mentioned paper, is, that the periodicity in the maturation of ova and of the attendant phenomena of menstruation is applied to most of the leading phenomena of nature which are dependent upon heat, and that like these it is influenced by seasons. This is ingenious, but wants more extended observations for its establishment.

35. *Discharge of matured ova, &c.* Besides proving that at each period of heat in animals ova are regularly discharged, whether sexual intercourse coincidently takes place or not, Bischoff has added some curious observations, showing that the bursting of the Graafian follicles, and consequent discharge of ova, is entirely independent of sexual orgasm, or of the influence of spermatozoa; thus: (*a*) Coitus may take place, and examination of the ovaries, six, eighteen, or twenty hours afterwards, prove that no ovum has escaped, although spermatozoa may have reached the ovaries. (*b*) Examination immediately after coitus may detect ova which have advanced two or more inches

along the fallopian tube, to have done which they must have started some considerable time before the act of coition. Bischoff considers it to be immaterial at what part of the fallopian tube the spermatozoa and ovum come in contact; so that they meet before the ovum has reached the uterus, impregnation is sure to take place. He states that he has clearly traced live spermatozoa up to the ovaries; this statement, and the inference he deduces from it, that fecundation of the ovum may take place at any part of the fallopian tube, is called in question by M. Pouchet, who has recently offered a table showing hourly the progress and condition of the spermatic fluid in the generative apparatus of the mammiferous female. His experiments were chiefly made on rabbits. From the 16th to the 25th hour after copulation, live spermatozoa were constantly found in the vagina and uterine horns. Even to the 21st or 23d hour these animalcules [?] continued very active, but shortly after that, they lost their vivacity, and died towards the 25th hour; so that nothing but broken-up tailless spermatozoa could be seen. He was never able to see live spermatozoa reach more than a very short distance up the uterine extremity of the fallopian tube; and he considers the observations of Bischoff and Wagner, who had found live spermatozoa on the ovaries, to be erroneous. According to his own view, it is in the uterus alone, or probably also at the mouth of the fallopian tube, that impregnation of the ovum can be effected. [To this it can only be said, that the positive evidence of Bischoff ought to receive more consideration that the negative statement of Pouchet, and that if the latter be correct in his views, it will be difficult to account for the mode in which extra-uterine or ovarian impregnation takes place, whilst the observations of Bischoff explain it at once.]

36. *Corpora lutea.* The great importance, in a medico-legal point of view, of determining the true nature of the "corpus luteum," and of ascertaining how far its existence could be safely relied on as proof that sexual intercourse and consequent conception had taken place, has of late years led to numerous investigations on the subject. Although the descriptions of corpora lutea given by most observers differ somewhat from each other, yet, on the whole, they so far correspond that, generally speaking, there can be no great difficulty in pronouncing whether or not a given substance found in an ovary be a true corpus luteum; such a one, at least, as is formed when a matured and discharged ovum has been impregnated. From Dr. Ritchie's observations, it would seem that one cause of the discrepant accounts given of the corpora lutea, may be found in the varying characters which these structures occasionally present; thus concerning the seat of the yellow deposit, about which there has been so much dispute, Dr. Ritchie states that it may be situated either on the exterior of the ruptured Graafian follicle, between its layers, or within its interior; he considers the yellow mass to be an hypertrophy of the granular layer lining the internal membrane of the follicle. Besides varying in seat, Dr. Ritchie states that the corpora lutea also vary a considerable degree in aspect and character, some being of a

white colour (*corpora albida,*) and either of a soft fatty aspect, or dense and shining; others (*corpora cephaloidea*) are yellowish, and of a brain-like character; and another class (*corpora rubra*) which are at first similar to the last, although they are plumper, more vascular, better developed, and the granular matter of which they are composed subsequently becomes of a decided red colour. There has always been considerable difficulty in deciding the question, whether or not the presence of a so-called true corpus luteum could be relied on as a proof of conception. Many have considered it to be an infallible test, and have based their evidence upon it, in medico-legal inquiries; but some late observations of Bischoff and Raciborski seem to prove, that not merely as a result of conception, but at each menstrual period, the discharge of an ovum was followed by the formation of a corpus luteum; in this conclusion, however, there is no doubt they were mistaken; and by the following recent observations, Raciborski admits that they were so, although Bischoff still maintains that what he has observed to take place in animals, namely, the formation of a true corpus luteum after each period of heat, takes place in the human female also; this offers a good instance of the danger of drawing conclusions from analogy alone, for although it appears certain that a corpus luteum is formed at each period of heat in animals, which is undistinguishable from that formed after conception, yet any one who has had frequent opportunities of examining human ovaries will be aware how seldom true corpora lutea are found in them, and that they only exist in cases of pregnancy, or where delivery has but recently taken place; whereas, if, as Bischoff supposes, a corpus luteum is fouud at each menstrual period, one ought to find several in almost all the ovaries of those who die during the age in which the habit of menstruation continues. The clots of blood in various stages of discolouration (according to their age,) so commonly found in ovaries, although they indicate a ruptured Graafian vesicle and the discharge of a matured ovum, yet they bear no resemblance or analogy to the yellow mass observed when the discharged ovum has become impregnated.

The following conclusions of M. Raciborski are especially important since they correct his previous statement that a corpus luteum is formed at each menstrual period in the human ovary, and that its existence cannot be regarded as a proof of conception. His present views confirm the observations of Deschamps.

1. The corpus luteum is the result of an hypertrophy of the granular layer lining the internal or proper membrance of the Graafian follicle; the only difference between the corpus luteum and this granular layer is that the granulations of the former are larger and more numerous, and contain many more yellow oil globules. 2. The transformation of the granular layer into a corpus luteum commences as soon as the ovum is matured and the follicle ready to discharge it. 3. As soon as the Graafian follicle is ruptured, the process of transformation becomes more active; but in the human female the degree of activity varies greatly, according as the expulsion of the ovum is

spontaneous, as during a menstrual period, or coincides with sexual intercourse, and is followed by conception. In animals this difference does not exist; whether expulsion of the ovum has been followed by coition or not, a true corpus luteum forms. In the human female, however, if expulsion of the ovum has not been followed by conception (as occurs at each menstrual period) the granulations certainly increase, but their activity of growth stops short when they have formed a thin, yellowish membrane, which lines the inner tunic of the vesicle, and is found to contain within it a clot of blood, more or less altered, according to the period at which it is examined. If, on the contrary, conception coincides with expulsion of an ovum, the granulations rapidly increase in size and number, and shortly form a yellow mass, quite filling the cavity of the follicle, or leaving at the most a white fibrinous-looking streak in the centre, indicating the existence of a former cavity. 4. In all cases this corpus luteum remains of its full size to the end of pregnancy; but after delivery it rapidly disappears, so that at the end of three months nothing but a small and almost colourless spot will remain. 5. It results from the above that by simple inspection, one can readily distinguish cases of simple spontaneous expulsion of ova from those in which the expulsion has been followed by conception.

37. *Structure of the uterus.* The following observations on the arrangement of the muscular fibres of the uterus, by M. Jobert de Lamballe, are extracted from the American Journal of Medical Sciences.

The fibres of the single muscle which forms the uterus are arranged in layers, and present the following direction: the longitudinal superficial fibres, which may be called median from their position, are seldom seen on the anterior surface, but are constantly met with on the posterior, where they constitute two superincumbent layers. 1. Posteriorly they begin at the fundus of the uterus, and end at the uterine extremity of the vagina, to which they become attached, with the exception of some few that terminate on the neck of the uterus; they adhere by one surface to the peritoneum, by the other to the oblique fibres. 2. The anterior superficial fibres do not pass along the entire extent of the uterine parietes, but cross each other before they arrive at the round ligament of the opposite side. Some contribute to form this ligament, whereas others pass behind, and terminate on the lateral regions where they cross those of the posterior region. 3. There are other superficial fibres, only evident during pregnancy, which are destined to the fallopian tubes, and to the ovarian ligaments. Some originate at the fundus of the uterus, unite to those which contribute to form the fallopian tubes, and pass on to the anterior part of the ovarian ligament; others, more numerous, originate from the posterior surface of the fundus, and pass on to the same ligament. Lastly, a few transverse fibres from the posterior surface, form its inferior part. The numerous fibres which pass on to the fallopian tubes originate at the fundus of the uterus, and form a thick fasciculus, which divides into two secondary fasciculis, destined

one to the ovarian ligament, the other, more voluminous, to the fallopian tube. Some fibres separate from the common fasciculus, and lose themselves in the cellular tissue which separates the fallopian tubes from the round ligament. The deep fibres are very visible when the uterus has undergone rather lengthened boiling. They all evidently present a semi-circular direction, are rather oblique, and only differ from those above described by their small size, and by their belonging exclusively to the body and to the neck of the uterus. They cross each other on the median line anteriorly and posteriorly, as also on the sides, so as to produce a kind of network. Their thickness varies as they approximate the internal surface of the uterus, where they appear to describe circles exterior to the internal membrane. There are annular fibres along the fallopian tubes, which do not entirely encircle it, and are deep seated. Lastly, the blood-vessels are encircled by fibres similar to the deep muscular layer which surrounds the intestinal canal. The uterine neck is formed by fibres which constitute semicircles, and decussate without mingling. The muscular fibres of the vagina are posteriorly continuous with the longitudinal fibres of the uterus, but anteriorly they terminate abruptly, at the junction of the vagina with the uterus.

In order to empty the uterus of its contents, the longitudinal fibres tend to diminish the length of the uterus, whilst the semicircular ones diminish its cavity in every other direction.

38. *Structure of the human placenta.* In giving an abstract of the following observations on the structure of the placenta by Mr. Goodsir, it will render the subject more intelligible to divide it into three heads, as adopted in the original memoir :—

1st. Each placental tuft consists of a trunk, of primary branches, and of secondary branches or villi. Each villus is made up of the following parts: (*a*) An *external* fine transparent membrane. This membrane is common to the whole tuft, passing from one villus to another, and closely covering the free surface of each. (*b*) A layer of flattened nucleated cells beneath this membrane, (*external cells* of the villus,) here and there these cells are grouped together into heaps, in the centre of which is a germinal spot, which is engaged in the constant formation of new cells.* It seems probable that the internal aspect of this layer or cells is lined by a fine membrane, as in the case of the intestinal epithelium. (*c*) Beneath these structures, and immediately surrounding the blood-vessels within the villus, is another still finer and more transparent, but firm and strong membrane, (*internal membrane* of the villus.) This is readily separable from the layer of cells described : the space between them is probably occupied by a peculiar fluid. (*d.*) Within this membrane are the blood-vessels of the villus, consisting of one or sometimes two vessels, which form a simple or contorted loop occupying the cavity of the villus ; they are derived from the umbilical arteries and veins ; they differ from capillaries in their large size, and from arteries and veins

*Vide nutritive centres.

in preserving the same mean diameter throughout: one such vessel occasionally passes from one to two or more villi, forming a loop in each, before it becomes continuous with a vein. (*e*) Between these vessels and the internal membrane are some other cells, nucleated and highly transparent, called the *internal cells* of the villus.

2d. (*a*) The substance of each tuft of the chorion is made up of nucleated cells of various sizes, containing a granular fluid. (*b*) The surface of the tuft is covered by a fine membrane, which consists of flattened cells united by their edges. (*c*) The free extremity of each villus of the tuft is bulbous, and consists of transparent cells arranged round a central germinal spot. These groups of cells are the active agents by which the villi grow. (*d*) As gestation advances, and the allantois becomes applied to the internal surface of the chorion, blood-vessels become developed within the villi, which then communicate with the umbilical vessels. (*e*) Thus, then, the villi of the chorion form the internal (or fœtal) portion of the placental villi, previously described,—the loops of vessels, internal cells, and internal membrane of which have their origin in the villi of the chorion.

3d. (*a*) When impregnation has taken place, the mucous membrane of the uterus becomes greatly developed; the epithelial or cellular secretion of its follicles becomes augmented, and the vascular network occupying the outer follicular spaces becomes increased in size and extent. By this means a new layer or membrane is produced, the *membrana decidua*, which consists of two portions, the thickened vascular mucous membrane and the non-vascular cellular substance secreted by the follicles. The former constitutes at a later period the *decidua vera*, the latter the *decidua reflexa*. (*b*) As the (impregnated) ovum reaches the uterus, the developed mucous membrane or decidua begins to secrete, the os uteri becomes plugged up with a portion of the secretion, and the cavity of the uterus is filled with fluid—around the ovum this secretion consists of spherical nucleated cells, which possess the power of undergoing further development after being detached from the germinal spots or membrane of the secreting organ. These cells around the chorion of the ovum come to constitute the *decidua reflexa*. (*c*) Thus the tufts of the chorion are imbedded in a mass of nucleated cells, which cells are constantly being secreted from the follicles of the uterus, and which in all probability contain within them as they become fully developed, the nutritive materials, which the absorbing cells of the villi of the chorion are constantly taking up for the nourishment of the ovum. This cellular secretion seems thus to be to the ovum of the mammal what the albuminous fluid is to the ova of oviparous animals. (*d*) As the ovum increases in size, the amount of nutriment absorbed by the cells alone, is not sufficient for its wants; the allantois becomes applied to the inner surface of the chorion, and blood-vessels become developed within the tufts and villi. The vessels of the decidua vera at the same time enlarge and assume the appearance of sinuses encroaching on the space formerly occupied by the cellular substance of the decidua reflexa, in the midst of which the villi of the chorion are imbedded. Thus

the lining membrane of the vascular system of the mother becomes the *external membrane* surrounding the villi of the placenta. It lines the whole placental cavity, passing from tuft to tuft, and villus to villus, forming in this way threads and bands of venous membrane, which are tubular and filled with cells. These cells are continuous in the one direction with the *external cells* of the placental villi, and in the other with the gelatinous cellular substance constituting the *parietal portion* of the placental decidua, which is in connection with the wall of the uterus. The *central portion* of the placental decidua consists of the external cells and external membrane of the placental villi.

It appears from the above:—

1st. That the placental tufts and villi are made up on the one hand by the tufts and villi of the chorion, comprising umbilical vessels, internal membrane, and internal cells; and on the other hand by the lining membrane of the maternal vascular system, with a layer of cells beneath it, comprising the external membrane and external cells—the first portion is peculiar to the fœtus, the latter to the mother.

2d, These external cells are the remains of the decidua reflexa; they are still continuous with the cellular substance of the parietal placenta, by means of the cells filling the tubular threads of venous membrane.

3d. The function of the external cells is to secrete from the maternal blood (from which they are separated only by the external membrane) the materials of nutrition destined for the fœtus; this function is analogous to the digestive one performed by the intestinal mucous membrane in extra-uterine life.

4th. The function of the internal cells or those belonging to the fœtus is to absorb through the internal membrane the materials secreted from the maternal blood by the external cells. This matter is then taken up by the umbilical vessels and carried away for the nourishment of the fœtus. These internal cells perform a function analogous to that affected in extra-uterine life by the absorbing chyle-cells of the intestinal villi.

5th. Hence the placenta discharges not only the functions of a lung, but also of an intestinal canal to the fœtus.

39 *Respiration of the embryo.* MM. Baudrimont and Martin St. Ange having continued their researches on the absorption of oxygen by ova during their embryonic development, found in all their experiments on the eggs of hens, turkeys, adders, lizards, and many species of batrachians, that there occurred in each a true respiratory process, consisting in the absorption of oxygen, and the exhalation of carbonic acid, nitrogen, and watery vapour.

[It would appear that this process is essential to the development of the embryo. The exhalation of carbonic acid and watery vapour is, no doubt, the result of combustion of carbon and hydrogen by means of the absorbed oxygen; the effect of which will be the generation of a certain amount of heat, independent of that afforded by the mother

in the case of birds, and will in part account for the elevation of temperature above that of the surrounding air, observed in some reptiles during incubation,]

40. *Meconic membrane.* Dr. Ridge, in a recent work, describes the existence, during fœtal life, of a membranous sac lining the mucous membrane of the whole alimentary canal, and serving for the "envelopment and security of the meconium." He gives it the name of "membrana meconii." What purpose could be served by such a membrane, which is not adequately performed by the intestinal mucus, it is difficult to conceive. Most probably a compact layer of such mucus investing a portion of meconium has deceived Dr. Ridge into the belief of having discovered a new tissue. Not one alone, however, for he mentions a "rete vasculare," which he describes as a net-work of the most delicate blood-vessels, situated between the membrana meconii and the true mucous membrane. The existence of this vascular network, and consequently of the office attributed to it, of maintaining the nutrition of the membrana meconii, is, we must confess, as improbable as the existence of the membrane itself.

§VIII.—*Miscellaneous Subjects.*

41. *Effects of extirpation of the spleen and thyroid gland.* From Mr. Bardeleben's experiments it results, that the animals which survive the extirpation of the spleen appear speedily to recover their health, and present no difference from those which have not undergone this operation. He never remarked that they were more voracious than other animals, though it has been said that voracity is generally produced. In no case was the organ regenerated, though Meyer of Bonn has observed this. After removal of the thyroid the health was not sensibly affected. In one rabbit, the venereal appetite was considerably augmented; a circumstance worthy of remark, seeing that it has often been asserted, that removal of that gland abolished the venereal appetite. The animal deprived of both spleen and thyroid gland presented no change in any function. This fact is in opposition with the opinion of Tiedemann, that the lymphatic glands and thyroid body performed the functions of the spleen when that organ was extirpated. Finally, some physiologists have advanced the opinion that extirpation of the spleen produced augmentation of the venereal appetite, but abolition of the procreative faculty. M. Bardeleben satisfied himself of the incorrectness of this opinion, by breeding with dogs which had both spleen and thyroid extirpated.

Professor Meyer of Bonn, has offered further proof that after extirpation of the spleen the small lymphatic glands in connexion with the splenic artery become enlarged, coalesce, and, in no long time, form masses of considerable size, which doubtless perform to a certain extent the functions of the extirpated organ. In ducks and hens ten months sufficed for the production of a glandular mass equal in size to the original spleen. This speedy formation of a new organ discharging the functions of a spleen, will account in part for the trifling subsequent derangement resulting from its extirpation.

42. ***Parasitic animalcules in the sebaceous follicles.*** M. Gruby

gives an account of these animacules, the existence of which was discovered by the late M. Simon.

In man this parasitic insect is met with most commonly in the sebaceous glands of the skin of the nose. It usually occupies the excretory duct of the gland, and when there is a hair, the insect surrounds it. Its head is directed towards the base of the gland, its tail towards the surface of the skin, its feet are applied to the internal wall of the excretory duct. The duct is usually dilated at the part where the animal is lodged. In young persons there are never more than two or four parasites in each gland, but in persons of twenty-five and upwards, there are from four to eight. In still older individuals there may be from ten to twenty, in which case almost all the sebaceous glands are affected. They are common to most persons whether in good health or in disease.

It is only in large numbers in the glands that any irritation results, and then they produce that red, elevated, and tender spot at the orifice of the duct so commonly seen about the nose. These parasites exist abundantly in dogs and produce a very formidable and contagious disease. M. Gruby gives an anatomical and zoological description of these animalcules.

43. *Influence of hot air on animal life.* The results of some late experiments by Magendie, prove that no animal can endure the temperature of its body being increased more than 9° F. beyond its natural state. Thus two rabbits, the natural temperature of whose body is 102° F., were severally placed in stoves, the one heated to a temperature of 140°, the other to that of 212°. After a short time the temperature of both rabbits rose to 111° F., that which was in the hottest stove being the first to attain that temperature. He repeated the same experiment many times on different animals, but in no case did the temperature of the animal increase more than nine degrees. The same occurred with birds. Having attained this increase of temperature, the animals and birds in every case soon died; their arterial blood was found dark, like venous blood, did not redden on exposure to air, and had lost its property of coagulating.

The increase of temperature seemed to be attained chiefly through the medium of the skin, for when the head of an aminal was confined in the heated stove, so that it breathed the hot air, the elevation of temperature was much less in an equal period of time than when the body was exposed to the heated air, and the head was out of the stove. Thus a dog, whose body was within the stove, but the head out, lived only twenty-two minutes; but another, whose head was within the heated stove, and the body out, lived forty minutes.

An animal placed in a dry heated stove loses weight, but the amount lost is proportioned to the length of time the animal remains within the stove, not to the degree of heat, and the loss is no greater at the temperature of 212° than at one of 140° in an equal space of time. In stoves heated with moist air, on the other hand, Magendie found that instead of losing they often gained weight. Death oc-

curred much sooner in these stoves than in the ones heated with dry air.

[The result of these experiments are especially interesting when compared with the observations made by M. Constantine James regarding the effects on the body of the hot moist air of the baths or stoves of Nero, at Pozzuoli. The greatest amount of heat which M. James could endure in the hot moist air of the passage leading to the springs was 122° F.: when he had arrived at that part of the passage where the atmosphere had attained this temperature, he was utterly exhausted, and had nearly lost his consciousness. He was almost suffocated at a temperature of 112° F., whereas, in the caves of Lastaccio, in which the heated air is dry, he was enabled to bear without much discomfort a temperature of 176° F.]

44. *Anatomy and use of the thymus gland.* On this subject we are fortunate in being able to refer to some recent researches by Mr. Simon, the value and importance of which has attained for them the high honour of gaining the first Astley Cooper prize. In the endeavour to place before our readers a brief abstract of this work, we shall omit the author's very concise and accurate history of the labours of former writers, and proceed at once to the discussion of the original portions of his labours.

The first appearance presented by the gland, as observed in the fœtal calf, is that of a simple tube lying along the carotid vessels, and exhibiting faint traces of commencing areolar tissue. The contents of the tube at this time are granular, but do not contain any distinctly formed corpuscles. Mr. Simon suspected that this tube was not the primary condition of the organ, but that it might exist at an earlier period in the more simple form of a string of primordial cells; he has not, however, been able to verify the suspicion. He refutes the opinion of Arnold, (*Lehrbuch der Physiologie*, tom. ii. p. 265,) that the thymus is a development of the respiratory mucous membrane, as well as that of Bischoff, that it is in some way connected with the thyroid gland. The development of the gland proceeds in the same manner as that which has been observed as the primordial tube of the true glands, that is to say, by the addition of diverticula, which spring from the sides of the tube. These diverticula, when they have arrived at three fourths of a sphere, themselves give rise to secondary bulgings, which again reproduce others, until at length by the repeated occurrence of the same process conjoined with a continued interstitial molecular increase, the organ attains the bulk and complexity of the structure exhibited by it in the mature state of the fœtus.

The researches of Mr. Simon confirm in the main the dissections of Sir A. Cooper, with respect to the existence of a central cavity; he thinks, however, that it has hitherto been supposed to be larger than it really is. They likewise accord with those of Haugsted, in reference to the period at which the thymus attains its greatest size, this being, not as is commonly supposed during intra-uterine life, but at a certain period after birth. This exact time it is not easy to ascertain,

as it is probable that it varies in different instances; it has, however, been laid down as a law by the author, that its bulk is inversely as the amount of mortality and consequent exhaustion of tissue, and its duration, therefore, dependent upon the period at which muscular activity becomes established. In reference to this point the author has arrived at the following results:—1st. During the period next succeeding birth, the activity of the thymus is remarkable; it increases considerably in size, becomes turgid with secretion, and its specific gravity is lowered by the greater fluidity of its contents. This first growth is far out of ratio to the general increase of the body. 2d. For several months it continues to increase at a diminished rate, and merely in proportion to the general growth of the body; its further enlargement ceases about two years after birth. 3d. From this time, during a *very variable* number of years, it remains stationary, and, supposing the individual to be adequately nourished, gradually assumes the structure of fat. 4th. The duration of its decay, and the epoch of its entire vanishing are still more uncertain; about puberty, it seems in most cases, to suffer its chief loss of substance, and to be reduced to a vestigiary form.

The first appearance of this organ before birth is supposed by anatomists to be as early as the fifth week after conception, but in the tenth week of pregnancy it is sufficiently perceptible to the naked eye. It, at this time, exhibits a distinct tubulo-vesicular structure. The third chapter of the work contains a description of the mature gland. Its mode of formation has been already alluded to; it remains only to mention the intervesicular structure and the contained fluid. The intervesicular tissue is a prolongation of the wall of the original tube, and consists of an indescribably fine membrane, over which a close capillary network is spread for the purpose of supplying materials for secretion. This secretion consists of a fluid in which, as was discovered by Hewson, microscopic corpuscles were seen to float. These corpuscles are circular discs of nearly the same size as the coloured particles of the blood. Their average diameter is $\frac{1}{3830}$ of an inch. They are marked by minute dots which are supposed to be molecules of fat in combinatian with fibrine or solid albumen.

The author gives three separate chemical analyses of the thymus fluid, all of which concur in demonstrating the error of the opinion that it was essentially a highly carbonaceous product. It is proved by them on the contrary that the fluid contains no more carbon than enters into the composition of muscle and blood.

The nerves of the thymus are derived from the inferior and middle cervical ganglions and from the cardiac branch of the pneumogastric nerve.

In the comparative anatomy of the gland, the author's researches have been very extensive, but our space will not allow of a repetition of the different tribes of animals in which he has caried on his investigations, we shall content ourselves with giving the following summary of the results to which they lead. 1st. The *presence of the gland* is co-extensive with *pulmonury* respiration. 2d. Its shape and

position are variable and unimportant. 3d. Its *size* and *duration* are, generally speaking, in proportion to the habitual or periodical activity of the animal 4th. Where it remains as a persistent organ (as in the hybernating tribes) it is one of the general reservoirs for the accumulation of nutritive material.

In further prosecuting the developmental anatomy of his subject, the author next passes in review the maphological history of the true glandular system, with which he contrasts that of the thymus and its analogues, the thyroid, supraenal glands, and the spleen. The principal difference between the two orders of organs appears to consist in the ultimate arrangement of their secreting cells, that of the true glands being distinctly cellular, that of the glands without ducts, consisting of the cytoblast alone, the involving cell-structure being only of exceptional formation. It is a curious fact, however, that in those animals in which the thymus becomes a permanent organ, the nucleus instead of being simply surrounded by aggregate molecules, as in the temporary state of the organ, is converted into a perfect cell. These different points are rendered plainly intelligible by the plates with which Mr. Simon's work is liberally illustrated.

We now pass on to the most remarkable part of the work, the physiology of the gland. It is thus stated by Mr. Simon:—

"It secretes into a closed cavity certain particular elements of nutrition, which are deposited differently under different circumstances, viz.:—1st, In most animals it occurs only temporarily, the secreted matter then presents itself under a *fluid form*, and closely resembles the liquor sanguinis in ultimate chemical composition. 2d. In some animals, after discharging this temporary function, it assumes one of greater permanency, the sequestration of material in the form of solid fat. In both cases, however, though peculiar, the function is especially the same, and consists in the laying by of nutrient material. How this is used up, Mr. Simon next proceeds to show. Here, however, we are called to notice a certain circumstance which is co-existent with both the temporary and the permanent function of the gland, viz., that in both, *waste of tissue* is at a *minimum*. In the younger animal, muscular activity, which mainly contributes to this waste, has not commenced; in the hybernating animal it is suspended. Now the waste of tissue being at a minimum, the pabulum for the support of the respiratory process, must be supplied from some other source."

This source Mr. Simon declares to be the nutritive matter laid up in the central cavity of the thymus as in a reservoir, and he therefore assumes the office of that gland to be that of sequestrating nutritive matter, whereby it becomes "a sinking fund of nourishment in the service of respiration."

We must apologise for this imperfect analysis of this really valuable publication; our excuse must be that it only came to hand while the preceding pages were in the press. We were anxious that no time should be lost in making our readers acquainted with its new and important contents, otherwise we should have postponed its consideration for our next volume.

Upon the subject of the use of the thymus gland, we have next to mention a recent essay by Dr. Picci, a review of which appears in the *Medico-Chirurgical Review*, of January, 1845; as will be seen, he recurs to a theory propounded nearly two thousand years ago. After glancing at the theories of his predecessors, Dr. Picci suggests that the use of the thymus is chiefly of a mechanical nature, viz., to occupy a certain space within the thoracic cavity, while the lungs remain unexpanded in the fœtus; and thus to prevent the ribs and sternum from falling in too much upon these vital organs. The size of the thymus is inversely as the volume of the lungs; and when the latter become dilated after birth by the admission of air into their cells, the former immediately begins to shrink and becomes atrophied. In truth, it is only in the adult that the thoracic parietes are moulded completely upon the lungs; for in infancy and youth it is rather the thymus that is, in their place, moulded upon the thorax. The situation of this gland, the very nature of its tissue, and the greater expansion and development of its inferior half are adduced as arguments in favour of this opinion, besides the well known circumstance that, in those new-born children in whom the thorax is very largely developed, the thymus continues to increase gradually even to the end of the second year, it deserves notice that all those animals, in which the lungs are similar to those in the human subject, are provided with this gland; whereas we find it entirely wanting in those which breathe by branchiæ and membranous lungs. In hybernating animals, also, the thymus exhibits alternations of enlargement and decrease, according to the state of the respiratory organs. In the amphibia it attains its maximum of development. The circumstance too of the gland being usually rather larger than ordinary in phthisical patients, may be mentioned as lending some probability to the view proposed.—*Half Yearly Abstract.*

Case of Calculus Vesicæ; Dilatation of the Urethra; Lithotrity. By Henry Crawford, Esq., Surgeon to the Infirmary, Canterbury. —The subjoined case, in which the operation of lithotrity proved unsuccessful, presents many points of surgical interest, on which account I present it to the notice of your readers.

Sarah Stone, aged 46, mother of thirteen children, was admitted into the Canterbury Hospital, October 24th, 1845. Her countenance was expressive of intense suffering; she complained of very painful and very difficult micturition, and distinctly described the sensation of a large heavy substance within her, which arrested the flow of urine, by falling against the orifice of the bladder, so that her nights were often passed in painful and fruitless efforts to relieve that organ. All the pelvic viscera suffered distressing sensations of weight and dragging, frequently aggravated into throes resembling those of parturition. The urine deposited an abundant mucous sediment, and was of an highly alkaline smell. Upon passing a sound into the bladder, a large calculus was immediately felt. She dated the commencement

of her sufferings *ten years* ago, but during the last five they have become much more intense. She was ordered:—

℞. Infus. Pareiræ, oz. xij,; Acid. Nitric. dilut., dr. ij; Spir. Ætheris Nit., dr. iij. M. Capiat unciam sextis horis.

℞. Solutionis Morphiæ Muriat., Tinct. Hyoscyami, utrq. m. xx.; Mist. Camphoræ, oz. iss. M. Fiat haustus hora somni sumendus.

After a few days I gradually dilated the urethra, by means of Weiss's dilator, until I could pass my finger and feel the stone, which was readily grasped by a pair of lithotomy forceps, but the distance to which the handles of the instrument were separated, made it evident that something further was necessary to render extraction possible. I felt inclined to adopt the operation advised by Mr. Liston, under such circumstances, viz., to notch the fibres of the sphincter vesicæ on each side, afterwards employing the dilator with the view of obtaining room to extract the foreign body entire. However, after consultation with my colleagues, it was agreed to avoid the use of cutting instruments altogether, and to reduce the size of the stone by lithotrity. Accordingly, on Friday, October 31st, I proceeded to inject the bladder, which could be only imperfectly effected, and with considerable pain, on account of the irritable condition of that organ. Upon withdrawing the catheter I introduced Weiss's screw-lithotrite, with which the stone was easily seized and crushed, its diameter being, according to the index on the blade of the instrument, about two inches and a quarter; several large fragments were at the same time comminuted with but little pain to the patient. During several following days the fragments passed off freely; she expressed herself much relieved of the obstruction to micturition; the urine was longer retained, of a more healthy appearance, with less of mucous deposit or alkaline smell.

Encouraged by these favourable appearances, and aware that much remained to be done before the bladder could be cleared, I renewed my operations on the succeeding Monday, Tuesday, and Wednesday, crushing many fragments, and removing a portion of the detritus by the use of the screw-scoop. The pain chiefly complained of on each of these occasions was that arising from the injection of the bladder. I desired all that passed should be preserved, but this was not done, as much of the detritus mingled with the urinary deposit in the form of impalpable powder, and she stated that large pieces escaped from the urethra during the act of defæcation. Some large fragments which were collected shewed the composition of the calculus to be triple phosphate, with here and there a layer of the lithates.

On Friday she complained that the passage of urine was again obstructed, and a large fragment was felt by the catheter, presenting at the internal orifice of the urethra. I endeavoured to push this back into the cavity of the bladder, in order to seize it with the lithotrope, but all my efforts were ineffectual. I then thought it would be possible, after moderately dilating the urethra, to remove this fragment with the lithotomy forceps, as I had attempted to do with the entire calculus in the first instance; this I put in practice on Saturday morn-

ing, and persevered for several minutes in gentle but firm attempts at extraction; the proceeding caused considerable pain, and I was at length reluctantly compelled to let go my hold.

On the Monday following she was very sick, which she attributed to the fright caused by an accident which had been brought into the ward on the previous day; vomiting of all ingesta succeeded, with pain in the epigastrium, which quickly extended over the whole abdomen, and she sank with all the symptoms of peritonitis, on Saturday, November 15.

Autopsy.—The peritoneal surface of the stomach and intestines was coated with recently effused lymph; the walls of the bladder were much thickened; its cavity contracted; its mucous membrane of a dark venous-red colour, and it contained *one* flattened irregular shaped portion of stone, weighing three drachms, but *no other fragments, or detritus*. The mucous membrane was not ulcerated, nor did it appear to be injured by the instruments employed.

Remarks.—The application of lithotrity to large stones has been strongly deprecated by some of our most able surgeons; but their observations have reference chiefly to the operations on the male; the facility afforded to the passage of fragments of considerable size through the short and dilatable female urethra, diminish the force of many of the above objections when applied to it. Had Mr. Liston's operation been adopted in the case of Sarah Stone, it may be doubted whether fatal consequences might not have followed the immense degree of dilatation required, not to mention the dreadful (though minor) evil of a permanent incontinence of urine. The unfavourable symptoms in the present case followed the second employment of the dilator, which occasioned more suffering than the lithotrite, though not carried so far as to produce even temporary incontinence of urine. The small amount of pain and temporary relief produced by crushing of the stone, together with the remarkable manner in which the bladder contrived to rid itself of so large a portion of fragments and detirtus in the above case, appear to indicate that nature is on our side in the employment of crushing, as a means of relieving the sufferer from vesical calculus; though I admit that the question remains open whether the safer, and therefore the preferable operation to have recourse to in another similar case, would not be the old one of lithotomy, through the vagina.—*Prov. Med. and Surg. Journ.*

Effects of obliterating the Fallopian Tube.—In two female pigs, subjected to experiment in the Veterinary College, the abdomen was opened, as in the common operation of spaying, and the horns of the uterus, together with the ovaria and Fallopian tubes, were freely exposed. In one case, a ligature was placed around the Fallopian tubes, close to the horns of the uterus; and in the other, a portion of each tube was removed. The operation was similar in effect in both—namely, disconnecting the uterus from the ovaria, the object in view

being to ascertain the result upon the desire for sexual intercourse, and the ability of the animal to propagate its species. The pigs were small when operated upon, and both did well, and grew to maturity without exhibiting any symptoms of disease. They were destroyed the day before yesterday, and we find in both instances the Fallopian tubes to be impervious. This state of parts seems not in the least degree to have lessened the desire for copulation, since when the animals arrived at puberty, the labia pudendi became inflamed, with other symptoms denoting œstrum, and the male was taken by both of them. A few weeks after, he was admitted a second time, and, on the animals evincing a desire for the male a third time, they were then slaughtered. In the ovaria, in each, the corpora lutea were distinct, being seemingly about to burst. In one, numerous hydatids were attached to the uterus, while, in the other, large sacs, containing a dark matter, resembling coffee-grounds, were adherent to the ovaria. The parts, also, on cutting into them, appeared unusually vascular, but we must remember, that the animals were destroyed during the existence of sexual excitement. The result of these experiments would seem favourable to the idea, of the passage of the sperma up the Fallopian tubes, in order to impregnate the vesicle of the ovarium; and it certainly proves, that the obliteration of the tubes does not prevent desire, although it renders propagation impossible.—*Mr. Spooner, in Veterinary Transactions* for Nov., as quoted in *Lancet*, 20th Dec. 1844.

Glass Pessary Broken in the Vagina.

January 14, 1846. Was called early this morning to visit Miss R. E. The patient is a maiden lady of about 40, and has been troubled for several years with prolapsus uteri, and for a long time has been obliged to wear a pessary. Those used have generally been of the gum elastic kind, which after being worn for two or three months, it was necessary to remove to cleanse, as the secretions which gathered about them became too irritating to bear. On this account, after attending to the removal and introduction of the instrument several times, I recommended the use of the *glass* pessary, as less irritating, less liable to collect the secretions, and more durable. Accordingly I procured and introduced one of size No. 2, Aug. 21, 1845. It answered the end designed, and gave rise to no inconvenience or trouble till this date. On arriving at her residence, she said she had not sent for me because she was sick, but because she was "scared." Inquiring the cause of her alarm, she told me that the pessary had broken—that while standing at the window, doing nothing, she heard a noise, and that any effort since had caused pain as of something pricking her. She could not account for it, unless, as she humorously remarked, it was *frosty!*—it being a cold morning. On examination I found it broken, indeed, into a great number of pieces. Parts of the periphery were in situ, and all the parts were at the upper part of the vagina. I found I had an unenviable

task before me—the extraction of these sharp angular and pointed pieces of glass from the vagina, lined with a delicate mucous membrane, lying in rugæ. I had some doubt of the feasibility of the operation, and some apprehension for the result. But I commenced operations, and after two hours and a half of diligent and most careful manipulations, I succeeded in extracting every vestige of the glass. At least, several examinations afterwards, in several positions of the patient, did not discover the least particle remaining. Notwithstanding the care used, however, the vagina was unavoidably somewhat lacerated, so that a little hemorrhage was produced. Perhaps a tablespoonful of blood accompanied and followed the operation. My own fingers were also cut a little. The central piece, which was entire and averaging 1½ inch in diameter, having two rims, of which the edges were very jagged and pointed, was the most difficult to extract. Fearing that severe inflammation might ensue, I prescribed the antiphlogistic treatment and regimen, and an opiate and astringent injection to be frequently thrown into the vagina.

16th.—A slight fever ensued, considerable pain of bowels, especially in moving. Also some dysuria and tenesmus were present. Patient complained of a sensation of pricking, which was supposed to depend upon a small piece of glass remaining. A most careful examination, however, did not discover any such thing. The antiphlogistic treatment and injections were continued. The prospect at present (Jan. 20) is that the affair will not result in anything serious.

The patient had a severe fall on the fundament about ten days previous, but the absence of any pain or uneasiness from the pessary would lead to the conclusion that the instrument could not have been broken at that time. The number of pieces of glass extracted was fifty, of all shapes and angles.

The inference from the above is very plain—that there is danger attending the use of glass pessaries. In future, in my own practice, I think I shall not employ them, so long as others can be obtained.—W.

Boston Med. and Surg. Journ.

Cases in Hospital Practice. By Jonathan Toogood, M.D., F.R.C.S.

Case I. *Amputation below the Knee: Secondary Hæmorrhage.*—John Mosse, of Woolavington, was admitted into the Bridgewater Infirmary for disease of the foot, for which the leg was amputated at the usual place, below the knee, on Tuesday. The stump was opened on the Friday following, when the whole surface was found in a state of sphacelus. The sloughs, which were superficial, began to separate the next day, in the afternoon of which some hemorrhage took place. No vessel could be detected and it soon ceased, but returned again on the following morning, when a most minute examination of the stump was made, but no vessel could be discovered; the whole surface was in a soft pulpy state. Quiet and cold applications were directed, and no further bleeding occurred that day; but at

twelve at night an alarming discharge of blood took place. As the patient was much sunk, it was determined to tie the femoral artery in front of the thigh; after this he lost no more blood, the stump soon assumed a healthy appearance, and healed in the usual way.

Case II. ***Amputation above the Knee: Secondary Hæmorrhage.***—A young woman was admitted on account of a disease of the knee-joint, of many years standing, which had greatly reduced her strength. The thigh was amputated in the usual way, and she went on favourably for ten days, when considerable hæmorrhage took place, which was arrested by cold; it recurred two days afterwards, when a vessel was tied, but the bleeding returning several times, although to no great extent, weakened her so much that it was considered advisable to tie the femoral artery as high up as possible. This was effected without difficulty, after which there was no interruption to her recovery.

Case III. ***Extensive Burn: Amputation at the Shoulder-Joint.***—James Ellis, aged 22, fell into a large fire on the hearth during an epileptic fit, and being alone, was not discovered until the right arm was burnt from the fingers to the shoulder, and the left to the wrist, some parts of each to a cinder. The abdomen, penis, breast, and back, were also extensively burnt. He was dressed, and removed the next day to the Infirmary. He refused to submit to amputation, but consented at the end of a week. There was scarcely room to perform the operation, and no chance of covering the stump, the integuments and muscles were burnt so close to the joint; besides which, the other injuries were so extensive, as to render the case very hazardous and unpromising. The artery was compressed with the thumb above the clavicle, and he lost but little blood, but it was necessary to tie several small arteries. He went on tolerably well, but on opening the stump on the fourth day, the whole surface presented a tawny sloughing appearance. It was dressed twice a day with red precipitate ointment, and bark, wine, and opium were liberally given. On the fifth morning after the operation, the left hand was removed above the wrist, after which the wound of the shoulder improved daily, and in seven weeks all the wounds were healed. For the next seven weeks he had no fit, but after that time the fits returned as frequently as ever. He lived for many years, and at last died suddenly.

Two other cases have occurred during my practice, and although the operation succeeded in both for a time, the patients eventually died from disease of the lungs.

Case IV. ***Carcinoma of the Breast: Operation.***—Numerous cases of disease of the breast have been presented to my notice, and amongst them many of a truly carcinomatous character. I have been frequently called on to remove them with the knife, and have often assisted others; but I never remember a case which terminated suc-

cessfully, although in some instances the operation has been undertaken under the most favourable circumstances. The event of the following case determined me never to advise the operation, although I would not refuse to perform it at the desire of the patient, after explaining fully the doubtful results.

A strong healthy woman, aged 50, had a large carcinomatous tumour in the breast, for which she had consulted several practitioners of eminence, all of whom agreed, as there was no appearance of the glands in the axilla or above the clavicle being diseased, in the propriety of the operation. I removed the whole breast from the pectoral muscle, and as no ligature was required, the parts were evenly brought together and healed in ten days completely. No constitutional disturbance followed, and never did a case promise a more successful termination. She continued perfectly well to the end of six weeks, when I was requested to see her on account of some uneasiness in the course of the cicatrix, which was attributed to the friction of her stays. On examination I observed a small pimple on the base of the cicatrix, which soon increased and became a troublesome sore, which rapidly spread, destroying all the parts around, and attacking the other breast. Her state soon became deplorable, and in less than six months she died.—*Prov. Med. and Surg. Journ.*

Case of Abscess of the Left Lung; thickening and partial Ulceration of the Pericardium; Adhesion of the Right Lung, and extensive Tubercular Deposition through the entire Pulmonary Structure; with Obscure Diagnosis. By A. B. Smith, M. D., of Ludlowville, N. Y.

Mr. Editor,—A very interesting article lately appeared in your Journal, by Dr. Swett, of New York, illustrative of the difficulty of correct diagnosis in some cases of pulmonary and cardiac affections. Should the following case appear to you to possess sufficient interest, in connection with the same subject, to justify its publication, it is at your disposal.

Campfield Tompkins, æt. 51, was a farmer of good health until about three years since, when, as the family believe, he received an injury by a fall from the scaffolding in a barn.

From that time until April, 1844, the period of his attack with the illness of which he died, he had occasionally, on rising in the morning, attacks of quite severe pain in the left hypochondrium. It, however, lasted but a short time, and left him feeling nearly as well as before, not interfering at all with the business of his vocation.

At the time above noted, while working in the field, he was attacked with severe pain in the left hypochondrium (that being the place to which he always pointed as the seat of most of his pain,) nausea, pain in the head and fever. Had a very restless night, was considerably worse in the morning. He however attempted to labor again, thinking thereby to get rid of his bad feelings, but he was soon obliged to desist, the symptoms becoming aggravated. Took a cathartic of sulph. magnesia which operated pretty freely, but procured no relief. The following day he called a physician, who gave a

cathartic followed with a relaxing diaphoretic; and left orders for the application of a sinapism over the seat of the pain. This removed the most urgent symptoms, but he was by no means convalescent, but from that time until his death, continued, with only transient periods of apparent convalescence, to decline.

From this time until March, 1845, when I first saw him, he had most of the time been under an alternative, antiphlogistic course of treatment—and often with considerable relief; worn tart. emetic pustulating plasters over the seat of the pain; complained of a tightness and fulness under the right breast, which soon succeeded his first attack, and continued to harass him the remainder of his life.

When I first saw him, he had the following symptoms:—a sallow, rather sunken countenance, tongue slightly coated with a yellow fur; pulse a little accelerated; skin preternaturally warm and dry; pain in the left hypochondrium; tightness and fulness under the right breast; bowels costive; urine scanty and high colored; also at times a slight hacking cough, attended with scanty mucous expectoration. Gave a cathartic, to be followed with blue pill every night, if the bowels did not remain sufficiently soluble, and a solution of tart. emet. and ipecac.; the latter to be discontinued when the febrile action subsided. Saw him three days after: sallowness mostly gone, tongue cleaning; no cough; tightness and fulness less frequent and distressing, and bowels regular. He sits up and walks about most of the time.

Prepared a decoction of *polygala senega, taraxacum, ipecac.* and *rhubarb*, which had no visible effect; did not supersede the use of the laxative pills, which we designed it should do. Discontinued this; prescribed *tinct. iodine*, to be taken and applied externally over the seat of the pain, and a pill of *mass. hydrarg., ipecac.* and *ext. taraxacum;*—to be taken every night on going to bed. Under this he appeared to improve, until he caught cold, when we had to discontinue the internal use of iodine, as it disagreed with his stomach and created a fever.

Until a short time previous to this, we had supposed the case to be one of impairment chiefly of the nutritive function, from a general glandular disease, in which the lungs were not seriously implicated, believing their morbid symptoms to be mostly sympathetic, from their mild and intermittent character. But we had to change our opinion somewhat by a closer examination, which revealed to us the following symptoms:—Countenance sunken; pulse sixty and quite feeble; tongue slightly coated with a white fur; slight hacking cough on rising in the morning, with but little and at times no expectoration; tightness and fulness periodical, occurring about half past one P.M., and remaining until about three—attended with some fever; pain in the left hypochondrium, dull and aching, with complete intermissions; but soreness constant and at times exquisite.

The right side of the chest was considerably enlarged, the ribs projecting about twelve lines in front of the sternum, very resonant, with a preternaturally strong respiratory murmur, though otherwise

healthy. The left side was much contracted, had no motion on respiration, elicited a dull sound on percussion, and could detect *no* respiratory murmur on the left of the mediastinum. From this we concluded that the left lung was totally impermeable to air, although we could not satisfy ourselves as to the cause. The heart's action was healthy as far as we could determine.

Some three days after this, the patient began more rapidly to decline—was very restless, though in but little pain; most of the time none at all—manifested nothing new, except that the tongue assumed the raw-beef appearance.

Being now certain that no medicine could reach his case, our treatment from that time was only palliative. With a little morphine, &c., he was quite comfortable, and rested pretty well at night. Had no hectic, unless the slight fever attending, or causing the tightness and fulness, might be so called. Had no night sweats or any other colliquative discharge, but continued gradually, and at times almost imperceptibly, to waste away, until the last sands of life had ebbed out.

Autopsy twenty-four hours after death:—Emaciation extreme; no discoloration, or tympanitis.

After opening the chest in the usual way, I commenced the elevation from the right lateral inferior part of the sternum and cartilages, with the following appearances, viz.:—Under the right breast, as low down as the fifth rib, commencing on the cartilage, and extending laterally, was an adhesion of the lung for about thirty lines; which, in addition to the pleura for its bond, had several cartilaginous bands. There were also several incipient tubercles in the inferior margin of the anterior lobe. With these exceptions, the right lung appeared perfectly healthy, had no tubercles, miliary or otherwise, in the summit. I then divided the diaphragm to the left lateral incision, without, however, opening into the left pleural cavity, as it was firmly adherent to the pericardium: the diaphragm was also much thickened. On extending the incision upwards through the mediastinum I found that much thickened, and also firmly adherent to the pericardium; and that again to the costal pleura, through the whole extent of its contact, except where extending ulceration had burrowed in. Through these openings, yellow and not very fœtid pus issued quite freely. Turning the sternum back, we found the left pleural cavity completely occupied with pus, the lung being entirely disorganized, and the bronchi obliterated. We also discovered near the ensiform cartilage, at the right margin of the sternum, a large tubercle imbedded in the pleura in the caseous stage, and two near the superior part of the sternum; one of which was quite small, situated upon the first bone of the sternum, and the other a very large one, being over twelve lines in its lateral diameter, upon the cartilage of the first rib. The *heart* was of the usual size and healthy. Being obliged to hasten the autopsy we made no further examination of the chest.

The stomach and bowels, with their appendages, were healthy, except the mesenteric glands, which had quite large patches of mili-

ary tubercles, with the interspaces healthy. The liver and spleen also contained the caseous tubercle, with, however, no other appearance of disease. In the appearance of the latter we were much disappointed, expecting from the symptoms, evidence of severe disease, the left hypochondrium having been the seat of most of the pain of which he complained. But doubtless this was sympathetic.

The singularity and much of the interest of this case consist in the absence of many, I might say most of the symptoms attendant on such a pathological condition, and the mild and equivocal character of those present. As far as I can learn, he never complained of pain or soreness in the chest (and with a view to detect the latter I have often thoroughly examined him,) had no dyspnœa; and never a harassing cough, and but little expectoration; slept perfectly quiet on either side; had no rigors,—or, as I conceive, hectic proper, and no night sweats. The character of the symptoms we will not again enumerate, as they have been sufficiently described.

Now in the light of these facts, the inquiry suggested to my mind, and one that I have not been able yet to satisfy is,—how could inflammation, sufficiently active as to cause hepatization and softening, have thus occurred in this occult manner and without producing more strongly marked symptoms?

I am sure that had there been an ordinary susceptibility of the system, disease could not have effected such extensive ravages, upon organs playing so important a part in the animal economy, and so essential to life, without having caused the most exquisite suffering, and giving evidence by unequivocal signs and symptoms of the character and extent of the disease. I question seriously whether such extensive disorganization could have thus occurred in any other than a system so devoid of sensibllity.—*New York Journal of Medicine.*

Pathological Anatomy.—Extraordinary Fœtus.—A male child was born with all the small intestines spread out on the abdomen. The delicate membrane, a prolongation of the peritoneum, which originally enclosed them as in a sac, had been ruptured in the delivery. The umbilical orifice, through which the intestines passed, with the navel string, was as large as a silver-mark (en sölv-mark,) and portions of them were so strongly adherent to the margin of the orifice, as to render their reduction impracticable. The child died twenty-eight hours after birth. On examination, no room for the intestines was found in the abdominal cavity, and all the parts were larger than ordinarily seen in a new-born child. That portion of the umbilical cord which traversed the sac, was very slender, but farther towards the placenta it was an inch in thickness.—*Lond. Med. Times.*

Pathology.—Curious Alteration of the Cornea in Hydrocephalus Acutus.—Dr. Stöber, of Strasburg, has observed, in several cases of hydrocephalus acutus, shortly before death, a peculiar alteration of the cornea of the eye. It consists in a semilunar yellow speck, situated at the lower margin of the cornea, without undue redness or vascu-

larity of the eye itself. This morbid alteration gradually extended upwards, when the disease was protracted, to the centre of the cornea, and terminated in ulceration. The lamellæ of the cornea were separated by a purulent infiltration, and pushed outwards, and in this way were detached from each other by the ulceration. Dr. Stöber regards this process as the consequence of the general debility. In one case, where the prostration of strength was considerable, he directed the instillation of opium drops (opiumo draáber) into the eye, and prescribed some doses of quinine, with a nourishing diet. These means were successful, and in ten days the ulceration had cicatrised.—*Ibid.*

Obstetrics.—Case of Vaginal Pregnancy.—On the 13th of January, 1839, the author of this communication visited a young woman, pregnant for the first time. He found her exceedingly debilitated, with a very feeble pulse, and scarcely able to articulate. For the last four months she had been constantly confined to her bed. About the fourth month of pregnancy, she had been attacked by strong bearing-down pains, and been relieved by an antispasmodic mixture. At this period a tumour made its appearance posteriorly in the vagina, and had since increased considerably in size. On examination, this tumour was found to be of the volume of a hat-crown, protruding between the thighs of the patient, and dragging forward the rectum. One arm was projecting from an orifice below the vagina, and from this it was judged that the fœtus was in a state of putrefaction. The arm was twisted off, and by the use of the hook, the fœtus was readily extracted from its containing cavity, care having been taken to pull from below upwards towards the patient's abdomen. The placenta adhered very firmly to the vagina, and from the dread of hemorrhage no attempt was made to remove it. The tumour was now very much reduced in size; aromatic fomentations, with acidulated decoction of cinchona, internally, were prescribed. Two days afterwards the patient died. The fœtus was of the male sex, and from seven to eight months old. In this case of extra-uterine pregnancy, the author assumes that the ovum had originally been introduced into the uterus, and had since, about the third or fourth month, dropped into the vagina, and there been developed to its ultimate volume.—*Ibid.*

Dangerous Effects from the use of the Tobacco Enema.—M. Bertini relates in the *Giornale delle Scienze Mediche della Societa Medico-Chirurgica di Torino*, the case of a child, four and a half years old, to whom its parents administered an enema prepared with part of a cigar, in the belief that the child was labouring under worms. When seen by M. Bertini, there was extreme agitation, convulsions, face pale and contracted, eyes fixed, expression of the countenance that of stupefaction, pulse small and weak, cold sweat all over the body, nausea, and the epigastric region painful on pressure. A purgative enema was immediately exhibited, and coffee without sugar given by the mouth. Friction was practised roughly all over the body; fric-

tion with sulphuric ether on the temples and epigastrium, and cold affusion on the face. The little patient soon began to amend, and, as soon as deglutition was possible, lemonade was exhibited freely. Two days afterwards the patient was quite restored.—*Ibid.*

The Yellow Colouring Matter of the Bile.—M. Polli read a paper at the Congress of Italian Savans at Naples last year, detailing certain experiments made by him on the yellow colouring matter of the bile, and on the colouring matter of the blood, from which he draws the conclusion that these two principles are identical in their nature, but differ in their degree of oxidation, that of the blood being at the maximum, and that of the bile at the minimum.—*Ibid.*

Castor Oil Seeds.—M. Parola detailed at the meeting of Italian savans at Naples, a series of experiments conducted by him to ascertain the best preparation of the castor oil seeds for internal use. The oil itself readily becomes rancid, and neither the syrup nor the emulsion is of general service. M. Parola, founding his statement on numerous chemical and clinical experiments, considers the extract and the ethereal tincture, but especially the latter, as the most certain and most efficacious preparations of the ricinus seeds. He concludes from his experiments: 1st, That the ethereal and alcoholic tinctures have a purgative action four times more powerful than that of the expressed oil, and that they have not a greater tendency to induce vomiting, nor are more irritating than the oil. 2d, That these preparations remain unchanged for a very long time in any climate or weather. 3rdly, That the ethereo-alcoholic extractive principle is comparatively less purgative than the pulp from which it is obtained, showing that the ricinus seeds contain a principle insoluble in alcohol or ether.—*Ibid.*

Pellagra.—According to the report of the commission appointed to examine into the causes of this epidemic, its hereditary nature is clearly ascertained, and also the influence of insolation in producing the cutaneous erythema. The effects of bad and inefficient diet are equally well marked.—*Ibid.*

Centaurine.—M. Gennaro Galani states that he has extracted the active principle of the lesser centaury, and finds it a good succedaneum for quinine.—*Ibid.*

Electrical Chorea.—By this title, M. Dubini describes a disease which he has met with thirty-six times, and of which he has not been able to find any account in works. Its principal phenomena consist of strong shocks, succeeding each other at determinate intervals, preceded by heat of the skin and an acceleration of the pulse, resembling fever, and which sometimes bring on paralysis of the limbs. The extreme rapidity of these shocks has led M. Dubini to designate the disease electrical chorea. They generally affect one side of the body,

and are quite independent of the will. The disease is always accompanied with great depression. The most frequent cause is fright or presence of worms. During the fit speech is lost, although the mind is unaffected; the tongue swells, deglutition is impeded, and an attack of apoplexy often terminates the scene. At the autopsy, the presence of worms in the intestines is discovered; tubercles in the lungs, a serous effusion under the meninges, and an injected condition of the brain itself are also met with. No one of these changes is always met with, so as to be looked upon as the actual cause of the disease. The actual cautery, narcotics, and bleeding have all failed in its treatment. The only medicines which seem to have been of service, are mercurial frictions, preparations of zinc, arnica, and valerian.—*Ibid.*

Statistics of Insanity.—There are 20,000 persons ascertained to be insane in England and Wales. This is, however, considerably below the actual number. They belong to every station in society; two-thirds, however, of the whole are objects of charity, and are maintained entirely at the public expense. There are 3790 private patients confined in asylums in England and Wales, out of which 1989 are males, and 1801 females. The number of paupers so confined is 7482; males 3532, females 3950; making in the aggregate of private and pauper patients, 11,272 confined in asylums in England and Wales. Out of the number above specified, 2519 are said to be "curable"—viz., 1045 private, and 1474 pauper patients. Incurable cases, 8736—males 4331, females 4405. Of epileptics there were 951, idiots 598, homicidal patients 278, suicidal cases 696. There were of the 11,272, married 3165, single 6328, widowed 1138, not known 409. Upper and middle classes 2704, agricultural 1652, artisan and in-door 3868, others 2816. Criminal lunatics 257, found lunatic by inquisition 233.—*Prov. Med. and Surg. Journ., from Dr. Winslow's Edition of the Lunatics' Act.*

Climate of Madras.—During the months of January and February, the weather at Madras is cool and pleasant; and this period is considered to be the most healthy season of the year. The mean temperature of these months is 76° F.; the wind blows steadily from N. E. and E., and the average fall of rain is 1 inch 25 cents. In March, April, and part of May, the south (or as it is called, 'the along-shore wind') prevails, and is reckoned very unwholesome, particularly to old residents, who generally suffer during this time of the year from rheumatic pains; the mean temperature of these months is 85°, and the average of rain 1 inch 85 cents. In the early part of May, very violent gales of wind have occasionally been experienced, accompanied with heavy falls of rain; about the middle of the month, the hot land-wind commences, and blows generally with great violence from about midnight till 12 or 1 in the day, when it is succeeded by the sea-breeze, which at this season is very refreshing; the land-wind continues throughout June and July, the mean temperature of these months being 88°, and the average of rain 2 inches

20 cents. In the beginning of July there are generally heavy showers of rain, which diminish the heat of the land-wind; but it continues to blow during the month, though with less violence: mean temperature 85°, average fall of rain 3.37. In the month of August and September the weather becomes cloudy, close, and oppressive; the sea-breeze being uncertain, and the winds generally light and variable, with frequent calms; heavy falls of rain, ushered in by thunder and lightning, also occur in these months, that the cholera has generally raged epidemically at Madras. About the beginning of October the N. E. monsoon commences, and continues through the months of November and December; in October heavy gales of wind are very frequently experienced: the weather is cool and damp, the mean temperature 80°, and the average of rain 30 inches.

Average Medium Temperature throughout the Year, for 10 *Years.*

1829	1830	1831	1832	1833	1834	1835	1836	1837	1838
Fahrenheit.									
3	81	81½	84	83	87	82	77½	82½	86½

Monthly Journal of Med. Science.

French Views of Medical Reform.—Congress of Medical Practitioners now sitting in Paris.—"In France there has long existed an *inferior* class of practitioners, denominated Officiers de Santé. The members of the *superior* class all possess diplomas in Medicine or Surgery. The 'Doctors in Medicine,' and the 'Doctors in Surgery,' receive precisely similar educations, undergo the like examinations, and are equally eligible to fill all the public medical offices and appointments. These constitute one grade of practitioners in France; the Officiers de Santé constitute the other or inferior order. Should the superior and inferior grades be maintained? That was the question which the Congress undertook to discuss; when, with characteristic boldness, it was resolved, without a dissentient voice, that *the inferior ought to be abolished.* Thus the medical senators of France have announced, not only to the government of their own country, but to all others, that the legislature of every nation is bound to furnish the commuuity with one well-educated and thoroughly qualified class of medical practitioners; and that a body of *inferior* acquirement, whether styled Officiers de Santé or Apothecaries, ought not to be recognised by the law. What will the manœuvring apothecaries of Blackfriars and Regent Street say to this decision?"—*London Lancet.*

M. Ricord on the Tertiary forms of Syphilis, and their treatment.—Tertiary symptoms may be said to be essentially characterized by not being transmissible hereditarily. When syphilis has arrived at

this stage, it seems to lose its specific character, and to constitute a cachexia, having a certain affinity to scrofula. I believe, it indeed, to be the most frequent cause of that malady. The constitution is deeply modified after passing through the different stages which intervene between the first manifestation of the primary sore and that of tertiary disease. The affiliation cannot always be traced, although, no doubt, it always exists. The secondary symptoms may not have been perceived, or may have been forgotten. Thus when transmitted by heredity, secondary symptoms appear within five or six months of birth, and subsequently disappear, whether treated or not. They may not be recognised by the parents; and yet tertiary symptoms supervening, the latter may appear to occur primarily.

Mercury may prevent the manifestation of tertiary symptoms, or modify them when they have appeared; but because it does not always prevent their occurrence, we are not warranted in concluding that it causes them, or that they are the result of the combined influence of mercury and syphilis, as has been asserted by many pathologists. They have been repeatedly observed on persons who have never undergone mercurial treatment—a fact which decides the question.

It is generally believed that tertiary symptoms are more serious and more difficult to cure in patients who have taken mercury than in those who have not; and among the former, more difficult to overcome, the greater the amount of mercury taken. The explanation of the fact may be easily given. The disease is most difficult to cure in such persons, because they are precisely those on whose economy syphilis has the firmest hold, and who have been, consequently, all along, the most rebellious to the action of mercury.

Tertiary symptoms are more serious than secondary, for the older syphilis is, the more formidable it becomes, and the more firmly does it establish itself in the economy which it deteriorates. Mercury, when it is not the most precious and the most efficacious of medicine, is a dangerous remedy, often, indeed, a violent poison. When it does not cure, it does not remain innocuous; on the contrary, it injures and debilitates the constitution. Now syphilis is the more formidable, the weaker the constitution on which it acts. In this sense, the abuse of mercurial preparations may be followed by deplorable results, as it places the patient in the most favourable circumstances for the development of tertiary phenomena. But although, in diminishing the quantity of fibrine in the blood, which mercury does, you weaken the constitution, you cannot, by such means alone, produce tertiary symptoms, with their peculiar characteristics. The caries which may be occasioned by carrying mercurial stomatitis to its last limits cannot be confounded with the caries of tertiary syphilis. Between the one and the other, there is the greatest possible difference.

Tertiary syphilitical symptoms rarely manifest themselves within six months of the primary infection; this is the inferior limit. As to the superior one, the longest term after which they may appear it is impossible to fix. No tertiary symptoms have been known to occur,

ten, fifteen, or twenty years after infection, or even after a still longer period. It rarely happens, however, when the interval is so lengthened, that the patient has not experienced some intermediate symptom. There has nearly always, been a thorn somewhere, some local ailment, notwithstanding that the general health has been excellent. Suddenly, a tibia becomes painful, a testicle swells or melts, or an exostosis appears. A syphilitical exostosis has, however, never been known to develop itself eight, ten, or fifteen days after the manifestation of a chancre. In some rare cases, I have seen patients at the third, fourth, or fifth month, present phenomena which occupied a medium position between secondary and tertiary syphilis, and which, subsequently, became evidently tertiary; but they were exceptional cases. Tertiary accidents are principally situated in the sub-cutaneous or sub-mucous cellular tissue, in the fibrous, osseous, cartilaginous, muscular, or nervous tissues, and in organs in their locality. One of the principal characteristics of tertiary syphilis is its tendency to concentrate itself in the internal organs. One of the earliest diseases of this period is syphilitical sarcocele.

Syphilitical sarcocele has also been called syphilitical albuginitis, and justly so, as it is the white tissues that are first affected. Generally speaking, it only attacks one side, although it is not very rare to see it begin simultaneously or successively in both testicles. The attack is scarcely ever preceded by symptoms which indicate what is about to take place. When, however, this is the case, acute pains and a sensation of heaviness are experienced in the region of the loins; these pains are sometimes nocturnal. Most frequently the patient is not himself aware when the disease begins to appear, his attention being accidentally directed to its existence. The disease commences generally by the body of the testicle, on which may be felt several small indurated points, which gradually extend from one to the other, often forming, as it were, zones or circles of induration. The indurations thus continue to extend and to coalesce until the whole testicle has been invaded by the malady. The testicle has then lost its elasticity, has become coagulated and hardened. At the same time there are no symptoms indicating inflammation, and the epididymis remains soft and free. The hardened testicle may continue of normal size, or it may increase in volume until it becomes two or three times as large as natural, or even larger. The syphilitical testicle is not lobulated, nor does it present mammillary eminences. Its surface may be uneven, but the unequal surface presents a certain regularity, and never rises into protuberances. When the testicle is very voluminous, the epididymis disappears, owing to its being spread out and flattened on the superior extremity of the enlarged organ. The spermatic cord also remains healthy and free from hardness. There is no pain in the testicle, nor in its vicinity, either when the testicle is left to itself, or when pressure is exercised upon it. The weight of the enlarged organ may, however, drag down the cord, and occasion pain.

The tumour is hard, solid, without elasticity, and offers to the finger

on pressure the sensation that would be produced by fibrous or cartilaginous tissues. The skin undergoes no alteration, but remains moveable, and of the natural colour. There is no febrile or functional reaction. The spermatic secretion diminishing gradually, especially if both testicles are attacked at the same time, the power of erection diminishes, and at last disappears entirely. The progress of syphilitical sarcocele is slow, essentially indolent; it may last not only several months, but several years. The tumour never suppurates, and shows no tendency to suppuration, nor does it ever go beyond the size I have mentioned. Arrived at a certain stage of its development, resolution commences, and a process sets in, the very reverse of the one I have described, either under the influence of proper treatment, or spontaneously. The testicle gradually regains its normal size, and the patient is delighted; but he soon becomes alarmed to find the diminution in size persisting until the testicle melts and disappears. As the testicle diminishes and becomes atrophied, the entire economy is altered, and the voice changes its tone.

The disease does not, however, always follow this course. There are cases in which cartilaginous, fibrous, or even osseous degeneration follow the syphilitical hypertrophy of the testicle. All the above changes may also take place in a limited point of the organ. In some exceptionable cases I have known syphilitical sarcocele to terminate by true resolution, the organ returning to its normal condition.

The disease, as I have stated, nearly always begins in the body of the testicle. I believe, however, that it may possibly commence in the epididymis in a manner analogous to that which I have described.

Can syphilitical sarcocele manifest itself in a person in whom the syphilitical diathesis has been transmitted by heredity? All that has been written on the subject of this disease has been drawn from the observation of adults who had been themselves infected, who had had chancres. It cannot, however, be denied, that syphilitical sarcocele, which is a late symptom—a tertiary one—may be the result of the hereditary transmission of syphilis, and yet only appear at the age of puberty, when the genital organs first begin to play a part in the economy. I believe, myself, that a host of testicular degenerations, which have hitherto remained unexplained, may be attributed to this disease, as, for instance, the atrophy of the testicle occurring in early life, and generally considered congenital, and many of the fibrous, cartilaginous, and osseous degenerations which we find described at every page of surgical works on the testes. All these, I firmly believe, are often only the results of syphilitical sarcocele transmitted by heredity. The same may be said of many cases of chronic orchitis, which, when not specifically treated, is often known to end in atrophy.

The causes which more immediately occasion the manifestation of syphilitical sarcocele are all those which, setting aside syphilis, are

likely to irritate the testicle. The most frequent of these is blennorrhagia—a blow, a contusion, will act in the same way. The tubercular, cancerous, or scrofulous diathesis, may contribute to bring to light the syphilitical diathesis. These affections may also, as a matter of course, complicate the syphilitical disease.

Syphilitical sarcocele may be complicated by all the known diseases of the testicle and of the annexed organs, by all the forms of hernia, by varicocele, by hydrocele, by acute or chronic epididymitis, by simple orchitis, by kysts, or by the various degenerations. When, however, it exists alone, the symptoms which I have above enumerated will suffice to enable you to establish a diagnosis. If there is any doubt, a trial of the specific treatment will throw light on the question.

The prognosis will depend on the freedom from complication, and on the duration of the disease. If it is uncomplicated, and of short duration, the specific treatment of tertiary symptoms may bring the testicle to a normal state, without leaving any trace of the disease. At a more advanced epoch, it is still possible to prevent degeneration, but atrophy will often follow, which is better than retaining a hypertrophied testicle. At a still more advanced period, when hypertrophy has set in, we must endeavour to prevent degeneration, which may be all we can accomplish.

Sometimes, in mixed cases, or cases in which the symptoms of cancerous or tuberculous degeneration complicate those of syphilis, so as to render it impossible to form a correct opinion of the share which each has in the disease, the specific treatment must be resorted to as a means of elucidating the question. Unless a testicle be unquestionably cancerous or tuberculous, it should never be attacked with the knife until mercury and the iodide of potassium have been tried.—*Ibid.*

LONDON HOSPITAL.

Hydrocele.—Difficult Diagnosis.—Henry S——, a tall man, aged forty-three, applied at the hospital in August, on account of a large swelling of the scrotum on the right side. The swelling was of a pyriform shape, and fluctuated, but by no means distinctly. It extended as high up as the inguinal canal, which was full and dilated. The testicle could not be felt, but at the back of the tumour there was an induration which indicated its site, the usual pain being experienced on pressure there. The tumour received an impulse on coughing—an impulse as distinct as is usually felt in scrotal hernia. On examining the tumour in the ordinary way, no transparency could be detected; but on placing the patient in a room from which daylight was carefully excluded, and examining the swelling through a tube, formed with a roll of writing-paper, a lighted candle being placed at the opposite side, transparency could be clearly recognised. The patient stated that he had been subject to a swelling of the scrotum as long as he could remember. Mr. Curling gave his opinion that

this was a case of hydrocele, the fluid being contained in a thickened sac, and an impulse on coughing being communicated to the swelling, in consequence of the sac extending as high up as the internal abdominal ring. He stated that the sac was of uneven thickness, and that, at its lowest part, there was a kind of pouch, the sac being thinner at this part, and the transparency, consequently, more evident. As a matter of precaution, he carefully cut down to the sac at this part with a scalpel, and made a small opening in it. About twenty-seven ounces of fluid, of a darker colour than usual, and thickly loaded with cholesterine, escaped. The testis was found rather large, and the epididymis and vas deferens a good deal thickened. Inflammation of the tunica vaginalis followed the operation, and at one time there was a prospect of the hydrocele being cured by it. But after some weeks, the fluid appeared to be returning, and the patient has since discontinued to attend.

Encysted Hydrocele of the Epididymus, combined with Varicocele, consequent on an Injury.—Difficult Diagnosis.—Relieved by Puncture and the Lever Truss.—Morris L——, aged forty-two, a stout German Jew, came under the care of Mr. Curling, as an out patient, in consequence of a swelling of the left testis. He attributed the origin of his complaint to a blow received six months previously. It caused considerable pain and swelling at the time, and he was admitted into the hospital, where he remained several weeks, and was then discharged. There was a soft doughy swelling at the upper and back part of the testis, and connected with it, and extending upwards to the inguinal canal, a large number of varicose veins; but the swelling was obviously something more than a tumour occasioned by dilatation of the spermatic veins. It felt irregular, and communicated to the fingers an indistinct sense of fluctuation; but on careful and repeated examination in a darkened room, not the slightest transparency could be detected. The part felt heavy and uncomfortable, and, at times, quite painful, so that the patient could not continue his usual occupation. Various external applications were used, such as mercurial and iodine ointments, and iodine paint, but without relieving the pain, or causing any reduction of the swelling, which remained for some weeks in a stationary condition. Suspecting that the swelling might partly consist of an encysted hydrocele of the epididymis, the transparency of which was completely obscured by the varicose veins distributed around the tumour, Mr. Curling introduced a grooved needle into the upper part, when the escape of a few drops of a clear pellucid fluid revealed the true nature of the case. An adjoining but separate and smaller cyst was afterward punctured in a similar manner. In a few days afterwards, the swelling had subsided a good deal, and the part was freer from pain, but it still felt heavy and uneasy at times. In about three weeks the swelling had increased to its former size, and it was again relieved in the same manner as before. Pressure at the external abdominal ring was found to relieve the sense of weight, and the uneasiness in the testis. The

moc-main lever truss was consequently applied, and the patient derived so much benefit from wearing it, that he shortly afterwards desired to be discharged.—*London Lancet.*

Extraction of Needles from the Flesh.—The following statement is taken from the Sheffield Mercury, and certainly, from what appears in the imperfect state of the report, presents some curious circumstances for consideration. The degree of credit to be attached to it we leave to our readers, premising only that one very remarkable circumstance, and seeming to indicate a more recent introduction of the needles than the correspondent of the paper seems to imply, is, that their polish was not at all affected. It most generally happens in such cases that the needles are corroded to a greater or less extent: "A striking illustration of one of this class of cases has occurred in Sheffield in the course of this week. The subject in this instance is a servant in the family of Mrs. Heppenstall, of Upperthorpe, and about 21 or 22 years of age. It seems the young woman was recently under the care of a medical man whose treatment produced salivation, subsequent to which she fancied that needles were slowly progressing from her left shoulder to the arm, at some depth in the muscular substance. On Monday the pain became excruciating, and presently one, and soon afterwards three small needles were extracted from the fleshy part of the arm; and on the following morning two more, with part of a third! They made their appearance partly above and partly below the elbow, and seem perfectly bright and uncorroded. Mr. John Heppenstall, who himself drew out the latter portion of the needles, says that they did not by any means easily come away; and that no blood followed from the puncture. Next in interest to the curious fact of the undoubted extraction of so many needles under the circumstances, is the question as to how they got in the young woman's flesh? Of this she can give no account, having no recollection of having either swallowed them or received them into her body from any external accident; but she says that sometimes she has felt the pricking of needles in her chest, while their progress down the arm was quite perceptible. She states also, that seven years since she recollects that not fewer than fifty similar needles and pieces of needles came from one of her fingers in the course of a fortnight."—*Ibid.*

Case of Lodgment and Retention of a Foreign Body in a Bronchus for the space of sixty years, with its final expulsion. (Communicated by Rodman Bartlet, Student of Medicine, Salisbury, Conn.)—The following case being somewhat unique in its character, may, perhaps, be deemed of sufficient interest to find a place in your valuable journal. It occurred in the practice of my preceptor, Luther Ticknor, M. D., who is ready to vouch for the facts here stated.

Richard Moore, of Salisbury, Conn., now aged sixty three years, inherited a good constitution, but when three years old, swallowed accidentally a piece of bone, which nearly suffocated him at the time,

and which, from that day to the present has produce a series of phenomena, which could only be accounted for, on the supposition that the bone was lodged in some portion of the bronchial tree. For a long time he was harassed night and day with a cough, and severe dyspnœa, attended with a rattling, or sibilant rhoncus, fœtid breath, more or less pain deep in the right side, about opposite the fifth or sixth rib, and midway between the sternum and dorsal vertebræ. These symptoms continued to harass him until nearly the age of puberty, when other symptoms appeared, of a still more alarming nature. At this period, the cough began to be accompanied with expectoration of purulent matter, tinged with blood; attended also with marasmus or atrophy, and other symptoms of confirmed pulmonary disease. At the age of nineteen, he was suddenly attacked with hæmoptysis, which continued, at irregular intervals, for a series of years; during which time his general health and strength were greatly impaired, so that he was incapable of much exertion, being, indeed, confined to the house for more than eight years. At about the age of twenty-eight, his general health began to mend; although still troubled with cough. For the next fifteen or twenty years, he was able to perform some labor, though not without considerable inconvenience from the symptoms above mentioned. In January, 1844, for the first time, since the third year of his age, the cough suddenly left him, and did not return for several months, when it again began suddenly to harass him as before. On the 8th of October, 1845, he experienced a remarkably uneasy sensation, of a pricking nature, deep in the right side, which excited violent coughing, and after one or two severe paroxysms, he experienced a sensation as if something had ruptured, or given way. This was instantly followed by the passage of something into the trachea, producing suffocation, which was forcibly ejected upon the floor, succeeded by the expectoration of purulent matter streaked with blood. On examination, the foreign body turned out to be a *bone* nearly three fourths of an inch in breadth, and one twelfth in thickness, oblong and somewhat triangular in form, smooth and convex upon one surface, the other covered with sulci and protuberances. It appeared that the bone which he had originally swallowed, from the traditionary accounts preserved in the family, was a splinter of a rib from which he was sucking the meat, and it seemed highly probable, on examining the piece ejected, that this also was originally a portion of the same bone, as the spongy lamellæ were still visible on one side, while the smooth convex surface of the other was equally manifest. Now, then, are we not authorized in believing that this bone was lodged in one of the rami of the right bronchus for this long period of 60 years, producing the phenomena which I have briefly described; including partial ulceration, whence proceeded the blood, and purulent matter, &c.? Your opinion on this point will be gratifying to one, at least, of your numerous friends and readers.

Salisbury, Conn. Nov. 15th, 1845.

[There are numerous cases on record where foreign bodies have been lodged in one of the bronchial tubes for several years, but we

know of no instance were a body of the size of that above described, has remained for so long a period in a bronchus, without causing fatal disease; and doubt whether a parallel case can be produced.—Ed.]
N. Y. Journal of Medicine.

On the Question whether Alkaline Carbonates exist in the Blood. By Baron Liebig.—The food of the carnivora contains only alkaline phosphates; that no alkaline carbonates, therefore, can be found in their blood, scarcely requires any special proof. But the case is altogether with respect to the graminivora, since their food contains a number of alkaline compounds with vegetable acids, which pass into their circulation, and are subsequently separated from the blood through the kidneys, and appear finally in their urine as alkaline carbonates. Now, if these alkaline carbonates exist as constituents of the blood of this class of animals, and if the elimination of carbon in the respiratory process is to be referred to their agency, it is obvious that we must be able to detect and to demonstrate their presence in the blood.

The following experiment was made to determine the question whether alkaline carbonates are present in the blood of graminivorous animals or not, and it must, I think, be deemed conclusive.

A mixture of between four and five pounds of the blood of an ox was boiled with twice its volume of water, and the coagulated mass formed subjected to powerful pressure. The fluid obtained was about the weight of the blood employed in the process; it had a powerfully alkaline reaction. Now, it is evident that this fluid must necessarily contain in solution any alkaline carbonates which existed in the blood.

On evaporation in a retort, (consequently not exposed to contact with the atmosphere,) until it occupied about forty cubic centimetres, it became a thick greenish-brown liquid, of the consistence of syrup, and had still a strong alkaline reaction. I took half this concentrated fluid, and placed it in a graduated tube in contact with carbonic acid gas, and allowed the mixture to stand for twenty-four hours, when I found the fluid had absorbed three times its own volume of the carbonic acid. Now it is evident, that if the capacity of this fluid for absorbing three times its own volume of carbonic acid gas depended on the presence of the carbonate of soda, and, consequently, the formation of bi-carbonate by the addition of the acid, the other half of the liquid obtained from the blood must evolve carbonic acid gas when acted on by a stronger acid, to the extent of about two-thirds that of the gas absorbed by the first half.

But this liquid, when brought into contact with hydrochloric acid in a bell jar over mercury, mixed with the acid without evolving the slightest perceptible trace of carbonic acid.

This experiment confirms the conclusions of Enderlin's analysis of the ashes of the blood of the graminivora—namely, that this blood contains no perceptible amount of alkaline carbonates.—*London Lan.*